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TENTH ANNUAL MEETING, APRIL 25 AND 26, 1929
ELEVENTH ANNUAL MEETING, MAY 1 AND 2, 1930
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INTRODUCTION

The American Geophysical Union is the American National Committee of the International Geodetic and Geophysical Union, and the Executive Committee of the American Geophysical Union is the Committee on Geophysics of the National Research Council. The object of the Union is to promote the study of problems concerned with the figure and physics of the Earth, to initiate and coordinate researches which depend upon international and national cooperation, and to provide for their scientific discussion and publication. In the accomplishment of this object, the Union is divided into sections following the plan of organization of the International Geodetic and Geophysical Union. Originally there were seven sections, namely, (a) Geodesy, (b) Seismology, (c) Meteorology, (d) Terrestrial Magnetism and Electricity, (e) Oceanography, (f) Volcanology, and (g) Geophysical Chemistry. The Section of Geophysical Chemistry was discontinued May 31, 1924, as the International Union had failed to provide such a section. At the meeting in May 1930 of the American Geophysical Union, a Section of Hydrology (g) was authorized to conform to the Section of Scientific Hydrology of the International Union. (Matters pertaining to scientific hydrology referred to the American Geophysical Union had been previously looked after by special committees on hydrology.)

Annual assemblies of the Union and annual meetings of its sections for the discussion of reports and symposia are held each year. Thus far there have been eleven such assemblies and meetings, all held in Washington, D. C. The transactions of these annual assemblies and meetings through the ninth have been published in the Bulletin series of the National Research Council as follows: Second, 1921, Bulletin No. 17; fourth, 1923, Bulletin No. 41; sixth, 1925, Bulletin No. 53; seventh, 1926, Bulletin No. 56; eighth, 1927, Bulletin No. 61; and ninth, 1929, Bulletin No. 68. The present Volume, published as one of the miscellaneous publications of the National Research Council, contains the transactions for both the tenth and eleventh annual assemblies and meetings held in 1929 and 1930, respectively. The first, third, and fifth annual meetings in 1920, 1922, and 1924, respectively, were devoted to discussion, there being no scientific papers presented; therefore, these transactions were not printed but were mimeographed for limited distribution only.

The tenth annual assembly of the Union and the meetings of its sections April 25 and 26, 1929, and the eleventh annual assembly and meetings of sections May 1 and 2, 1930, were held in the building of the National Academy of Sciences and National Research Council at Washington, D. C.

The tenth general assembly was held jointly, following a short business session of the Union, with the Section of Oceanography to hear general-interest papers which had been prepared for the program of the Section of Oceanography in addition to a number presented the preceding day at the Section meeting. The Section of Geodesy discussed progress-reports on gravity-work in Mexico and in Europe and America and on geodetic work in Canada and in the United States, and papers on recent developments of geodetic methods, instruments, and computations. In the Section of Seismology the papers were on surface-waves, the seismicity of the arctic, the forces and movements at the earthquake-origin, and the practical application of earth-vibrations from dynamite blasts. The Section of Meteorology discussed the report of the meteorological division of the Committee on the Physics of the Earth of the Division of Physical Sciences of the National Research Council The Section of Terrestrial Magnetism and Electricity held a symposium on physical theories of magnetic and electric phenomena. The Section of Volcanology heard communications on volcanic oceanic islands and the volcanic history of the San Juan Mountains of Colorado.

At the eleventh general assembly, in addition to a series of six papers on

"The utility of geophysics" in geodesy, seismology, meteorology, terrestrial magnetism and electricity, oceanography, and volcanology, the Union was addressed by Sir Hubert Wilkins on "The advisability of geophysical investigation in the arctic by submarine." Progress-reports on work in Canada, Mexico, and the United States, on methods for the precise measurement of time and determination of gravity, and on correlative studies of latitude-variations featured the meeting of the Section of Geodesy. In view of common interests, the Section of Seismology, after a short business session May 1, met jointly with the Eastern Section of the Seismological Society of America on the morning and afternoon of May 5 at the Bureau of Standards and May 6 at the Georgetown University; six of the papers were presented by members of the Union and a symposium on the publication of earthquake data was led by Harry Fielding Reid of the Union. The Section of Meteorology discussed the relation between the Section of Meteorology of the International Geodetic and Geophysical Union and the International Meteorological Committee, as well as reports on units of time, solar radiation, details of instruments, data required for indicating the climatological character of a region, and a final report on the Bulletin on Meteorology in the series "Physics of the Earth" to be published by the National Research Council. The papers given before the Section of Terrestrial Magnetism and Electricity dealt with the proposed International Polar Year 1932-33, the significance and importance of continuance of magnetic and atmospheric-electric observations on the oceans with accounts of instruments suitable for work at sea, secular variations in the United States and over the Earth's surface, and possible relationship of earth-movements and terrestrial-magnetic variation and of the aurora and the Earth's magnetism. The Section of Oceanography had two sessions, one May 1 and a second May 2, hearing papers concerned with oceanographic work in its various phases and particularly with relation to physical oceanography and its relations to marine biology and meteorology. The program of the Section of Volcanology dealt with volcanic activity and the central African volcanoes.

The Union was favored at both the tenth and eleventh annual meetings by papers from members of the national committees of Canada and Mexico reporting on the progress of governmental activities and on results of geophysical researches stimulated by the committees. The active spirit of international cooperation on the part of Canada, Mexico, and the United States was again well evidenced in these papers and by the attendance of many visitors during the two annual assemblies.

Opportunity is taken on behalf of the Executive Committee of the Union to thank most cordially the speakers, guests, and members of the Union for their active interest and participation, making possible the success of the meetings, as well as for the promptness with which manuscripts were submitted for publication.

As heretofore, the National Research Council has generously supplied funds for publication. Thanks are due J. J. Capello, who so carefully prepared the final manuscript for direct reproduction in this Volume.

J. A. Fleming,

General Secretary

STATUTES AND BY-LAWS OF THE AMERICAN GEOPHYSICAL UNION

(As amended to July 1, 1930)

PREAMBLE

The American Geophysical Union is hereby established by the National Research Council. It supersedes the "American Section of the proposed International Geophysical Union." The American Geophysical Union is the American National Committee of the International Geodetic and Geophysical Union and the Executive Committee of the American Geophysical Union is the Committee on Geophysics of the National Research Council. The American Geophysical Union is constituted as follows:

ARTICLE 1

Objects

(a) To assist in carrying out the objects of the International Geodetic and Geophysical Union, which are: To promote the study of problems concerned with the figure and physics of the Earth; to initiate and coordinate researches which depend upon international cooperation, and to provide for their scientific discussion and publication; and to facilitate special researches, such as the comparison of instruments used in different countries.

(b) To promote and coordinate the study of the various branches of geodesy and geophysics in the United States of America and its outlying territories.

ARTICLE 2

Membership

The membership of the American Geophysical Union shall be as follows:

(a) Those who composed the membership of the "American Section of the proposed International Geophysical Union" on July 1, 1919.

(b) Those who may be appointed by the Executive Board of the National Research Council upon nominations submitted by the Executive Committee of the American Geophysical Union.

(c) The Chairman of the Division of Foreign Relations, the Chairman of the Division of Physical Sciences, the Chairman of the Division of Chemistry and Chemical Technology, the Chairman of the Division of Geology and Geography, and the Chairman of the Division of Biology and Agriculture, of the National Research Council, members ex officio.

(d) The American officers of the International Geodetic and Geophysical Union and of its Sections, members ex officio.

ARTICLE 3

Sections

The American Geophysical Union shall be divided into Sections, in accordance with the organization of the International Geodetic and Geophysical Union, as follows: (a) Geodesy; (b) Seismology; (c) Meteorology; (d) Terrestrial Magnetism and Electricity; (e) Oceanography; (f) Volcanology; and (g) Hydrology.

The members shall designate the Section, or Sections, to which they will belong. The functions of the Sections, in their respective fields, shall be the promotion of the objects of the American Geophysical Union.

ARTICLE 4

Officers of the American Geophysical Union

The Officers of the American Geophysical Union shall consist of a chairman and a vice-chairman, elected to serve for a period of three years, and not to be eligible for immediate re-election to their respective offices; and a general secretary, to serve for a period of three years.

ARTICLE 5

Officers of Sections

The Officers of the Sections of the American Geophysical Union shall consist for each Section of a chairman, a vice-chairman, and a secretary, whose eligibility and periods of office shall correspond to those of the Union. The Chairman of each Section shall have the power to appoint, with the right to vote, a representative of the Section to such meetings of the Executive Committee of the Union as he may be unable to attend.

ARTICLE 6

Executive Committee

The Executive Committee shall consist of the chairman, the vice-chairman, and the general secretary of the American Geophysical Union, and of one representative from each of the sections of the Union, who shall be the chairman thereof or a member appointed by him; and, in addition, ex officio, the American officers of the International Geodetic and Geophysical Union, and of its Sections, the chairman of each of the Standing Committees of the American Geophysical Union, and the chairman of each of the following Divisions of the National Research Council: Foreign Relations, Physical Sciences, Chemistry and Chemical Technology, Geology and Geography, and Biology and Agriculture. This Committee shall have charge of all administrative matters of the American Geophysical Union, and shall be the representative of the Union in its relations with the National Research Council and with the International Geodetic and Geophysical Union. The Executive Committee, in consultation with the Sections, shall nominate to the Executive Board of the National Research Council delegates, or representatives, to the meetings of the International Geodetic and Geophysical Union and of its Sections, in accordance with the regulations fixed by the International Research Council for the voting power of the United States of America.

ARTICLE 7

Annual Meetings

The American Geophysical Union and its Sections shall meet annually at such time and place as may be chosen by the Executive Committee of the Union. The officers of the Union and of the Sections shall be elected at this annual meeting.

ARTICLE 8

Special Meetings

Special Meetings of the American Geophysical Union, or of its Sections, may be called when necessary by the respective chairmen.

ARTICLE 9

By-Laws

The American Geophysical Union is authorized to enact such by-laws as may be necessary for the performance of its functions and duties.

ARTICLE 10

Amendments

Amendments to the Statutes of the American Geophysical Union shall be subject to the approval of the National Research Council.

BY-LAWS

The fiscal year of the American Geophysical Union shall conform to the fiscal year of the National Research Council.

Each member of the Union, except those specified in group (c) of Article 2 of the Statutes, shall pay dues not to exceed $5.00 a year, the amount for each year to be fixed by the Executive Committee of the Union.

Standing committees shall be appointed for the ensuing fiscal year by the Chairman or the Chairman-Elect of the Union prior to the beginning of each year as follows: Statutes and By-Laws; Membership; Meetings; Budget; and Editorial and Publication.

All committees of the American Geophysical Union shall terminate at the end of the fiscal year for which they may be appointed.

Meetings of the Union shall be called at least thirty days in advance, and those present at the meeting shall constitute a quorum.

Meetings of the Executive Committee shall be called by the Chairman, and those present shall constitute a quorum.

Final action shall not be taken by the Executive Committee on matters affecting any Section in the absence of the Chairman of such Section, or his representative.

No mail ballot shall be closed until at least thirty days after the date of issue.

After thirty days' notice has been given, amendments to the Statutes (all of which are subject to the approval of the National Research Council) may be voted on either by mail or at a regular meeting of the Union.

Amendments to the By-Laws may be proposed either by mail or at a regular meeting; when voted on by mail they require for adoption a majority vote of the members; when voted on at a regular meeting they require for adoption a three-fourths vote of the members present.

Any amendment to the Statutes or By-Laws proposed by at least five members shall be submitted for vote to the members of the Union by the Executive Committee.

A member of the American Geophysical Union may be dropped from the Union by a three-fourths vote of the members of the Executive Committee.

Statutes and By-Laws adopted by a Section shall not conflict with the provisions of the Statutes and By-Laws of the Union.

Matters pertaining to a Section or Sections, and not pertaining to the Union as a whole, shall be acted upon by the Section or Sections concerned.

RESOLUTIONS

TENTH ANNUAL GENERAL MEETING

(Washington, D. C., April 26, 1929)

The topographic and control surveys of the United States

WHEREAS, A knowledge of geographic positions, elevations, and the configuration of the ground is essential in carrying on effectively investigations in geophysical and biological sciences, and these data are provided by the topographic maps executed by the United States government, which are based on the first and second-order triangulation and leveling also done by the federal government, and

WHEREAS, The present status of the topographic and control surveys of this country is not such as to meet the needs of science, engineering, and general industry, be it

RESOLVED, That the American Geophysical Union recommend that the officials of the federal government give serious consideration to the mapping situation of the country and take such steps as may lead to the improvement of conditions, and be it further

RESOLVED, That copies of this resolution be sent to the President of the United States, the Secretary of the Interior, the Secretary of Commerce, the President of the Senate, and the Speaker of the House of Representatives.

ELEVENTH ANNUAL GENERAL MEETING

(Washington, D. C., May 2, 1930)

James Percy Ault

WHEREAS, Mindful of his perfect worthiness to be held in high and universal esteem in the memory of geophysicists by reason of the cardinal contributions which he made to the magnetic survey of the Globe and to the materials of oceanography and marine meteorology, be it

RESOLVED, That the American Geophysical Union, in general meeting assembled, set down in record this expression of its sense of deep bereavement in the death of Captain James Percy Ault, while leading an expedition in the ship Carnegie for the purpose of extending the range of the facts of observation in geophysics, and be it further

RESOLVED, That copies of this resolution be sent to Captain Ault's family, to the President of the Carnegie Institution of Washington, and to the Acting Director of its Department of Terrestrial Magnetism.

Francis Anthony Tondorf

WHEREAS, The members of the American Geophysical Union have learned with profound regret of the death of Reverend Francis Anthony Tondorf, S.J., and recognizing his important contributions to seismology through the establishment and operation of a first-class seismological station at Georgetown University and his eminent services in keeping alive interest in and advancing earthquake investigation in the United States during a period when its importance was not yet generally appreciated, be it

RESOLVED, That the American Geophysical Union express the deep sorrow of its members and also their realization of the loss to science and especially to seismology, and be it further

RESOLVED, That a copy of this resolution be sent to the President of the Georgetown University.

Carey Vandervort Hodgson

WHEREAS, The members of the American Geophysical Union have learned with deep regret of the death on May 19, 1929, of our associate, Carey Vandervort Hodgson of the United States Coast and Geodetic Survey, therefore, be it

RESOLVED, That the American Geophysical Union desires to place on record its appreciation of the valuable services to geodesy rendered by Carey Vandervort Hodgson and to express its deep sense of loss both on account of his scientific work, his many endearing personal qualities, and his spirit of ready cooperation with his fellow workers, and be it further

RESOLVED, That a copy of this resolution be sent to Mrs. Hodgson.

Harry Crawford Frankenfield

WHEREAS, The American Geophysical Union, and especially its Section of Meteorology, suffered a great loss in the untimely death of Harry Crawford Frankenfield due to an accident during the past year, be it

RESOLVED, That the American Geophysical Union places on record its esteem of his high character, his devotion to his work as a weather forecaster and in gaging water flow and floods in our great river systems, and his attainments in the science of meteorology generally, and be it further

RESOLVED, That a copy of this resolution be transmitted to Mrs. Frankenfield.

Loss of the ship Carnegie

WHEREAS, The disastrous explosion on the ship Carnegie on November 29, 1929, in Apia harbor, Samoa, caused the death of the commander and of one member of the crew and injury to five others, and terminated, by the complete destruction of the vessel, one of the most important of oceanographic expeditions of modern times, and

WHEREAS, The untimely death of the vessel's commander, Captain James Percy Ault, caused irreparable loss to oceanography, and

WHEREAS, Captain Ault was a man both of the highest scientific ideals and capacity, as was shown by the plan for the expedition of the Carnegie and the skill with which it was being executed, and of the finest personal character, respected and loved by all who knew him, therefore, be it

RESOLVED, That the members of the Section of Oceanography of the American Geophysical Union do hereby express their grief and regret over the tragedy and the loss both to science and to the family and friends of Captain Ault, and be it further

RESOLVED, That copies of this resolution be sent to Captain Ault's family, to the President of the Carnegie Institution of Washington, and to the Acting Director of its Department of Terrestrial Magnetism.

Physical oceanography in all oceans

WHEREAS, In view of our present inadequate knowledge of the physical oceanography in all oceans, especially in the vast expanse of the Pacific Ocean, and in view of the unexpected and regrettable interruption of the extremely important scientific work so successfully inaugurated by the Carnegie, be it

RESOLVED, That the American Geophysical Union urges all organizations interested in the science of the seas to cooperate in securing the means necessary to assure the vigorous continued prosecution of this work, and be it further

RESOLVED, That copies of this resolution be sent to the Secretary of the United States Navy with copies for the Hydrographic Office, the Naval Research Laboratory, the Naval Observatory, and the Bureau of Aeronautics; the United States Coast Guard; the United States Coast and Geodetic Survey; the United States Bureau of Fisheries; the Oceanographic Committee of the National Academy of Sciences; the Woods Hole Oceanographic Institution; the Scripps Institution of Oceanography of the University of California; the Museum of Comparative Zoology of Harvard University; the Buffalo Museum of Science; the Carnegie Institution of Washington; the International Committee on Oceanography of the Pacific of the Pacific Science Association with copies for transmission to each chairman of the national committees; the British Admiralty; the Deutsche Seewarte; the Hydrographic Office of Japan; the Marine Biological Laboratory of the United Kingdom; the Institut für Meereskunde; the International Fisheries Commission; the Section of Oceanography of the International Geodetic and Geophysical Union; and the International Hydrographic Service.

Jubilee International Polar Year 1932-33

WHEREAS, The International Committee of Meteorological Directors has through its Commission on Terrestrial Magnetism and Atmospheric Electricity and its Commission for the Réseau Mondial and Polar Meteorology proposed a Jubilee International Polar Year for 1932-33, this being the fiftieth anniversary of the first International Polar Year, and has appointed a Commission for the Polar Year 1932-33, and

WHEREAS, The scientific work planned for the stations proposed during this polar year is of large geophysical interest particularly in terrestrial magnetism, atmospheric electricity, and meteorology, therefore, be it

RESOLVED, That the American Geophysical Union favors the proposals for an International Polar Year 1932-33 and recommends that the International Geodetic and Geophysical Union supports the undertaking, and be it also

RESOLVED, That the American Geophysical Union strongly indorses the proposal that the United States Government take part in the establishment of certain stations for the Jubilee International Polar Year 1932-33, and be it further

RESOLVED, That copies of this resolution be sent to the President of the United States, the President of the Senate, the Speaker of the House of Repre-

sentatives, and to the executive officers of those departments of the Government of the United States which are concerned in the scientific character of the work which is proposed for the Jubilee International Polar Year 1932-33, as well as those private research organizations which are interested, and to the International Committee of Meteorological Directors, its Commission on Terrestrial Magnetism and Atmospheric Electricity, its Commission for the Réseau Mondial and Polar Meteorology, and its Commission for the Polar Year 1932-33.

Additional seismological station in South America

WHEREAS, It is important that additional seismological stations equipped with modern instruments be established in South America, and

WHEREAS, The Magnetic Observatory of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington at Huancayo, Peru, because of its trained personnel, is in a position to operate seismographs and transit promptly reports of earthquakes without material additional expense, and

WHEREAS, A station at this Observatory, because of its southerly geographical position, would furnish data of great importance in the location of epicenters and for other seismological investigations, be it

RESOLVED, That the American Geophysical Union expresses the hope that the Carnegie Institution of Washington may find it possible to equip the Huancayo Observatory with modern seismological instruments.

SECTION OF GEODESY

(Washington, D. C., April 25, 1929)

GRAVITY-STATIONS IN MEXICO OCCUPIED BETWEEN JANUARY 1, 1926 AND DECEMBER 31, 1928

Manuel Medina

(Translated by W. D. Lambert)

Observations of gravity were made at 18 stations in Mexico between January 1, 1926 and December 31, 1928. (The positions of the stations and the values of gravity obtained during this period are included in the table accompanying the report submitted May 1930 by Dr. Sanchez to the Section of Geodesy as published in this Volume; descriptions of the gravity-stations will be found in the "Memorias annuales del Departamento Geográfico de Campo" of the Dirección de Estudios Geograficos y Climatológicos.)

Description and use of apparatus--At these gravity-stations pendulum outfit No. 9604 of type developed by the Coast and Geodetic Survey, made for us by Berger and Sons of Boston, was used exclusively. Chronometers and radio apparatus were used for measuring time

To check the invariability of the pendulums we have been accustomed to make determinations by means of gravity-circuits; that is, at the beginning of a series of gravity-determinations, observations are made at Tacubaya to determine the period of oscillation for each of the pendulums A, B, C. A like determination is made at the end of each circuit. The invariability may be judged by the following table, which shows the results obtained at the base-station in Tacubaya from 1923.

Table 1--Observed period of one oscillation at Tacubaya, D.F., Mexico

Series	Pendulum			Date
	A	B	C	
	s	s	s	
I	0.5015476	0.5014919	0.5014427	Mar. 1923
II	5471	4918	4420	Feb. 1925
III	5460	4896	4414	Nov. 1925
IV	5490	4922	4436	Feb. 1926
V	5497	4944	4448	Nov. 1927
Va	5480	4906	4419	Jan. 1928
	5476	4908	4419	Feb. 1928
VI	5472	4897	4406	July 1928
VII	0.5015525	0.5014944	0.5014459	Oct. 1928

In determining the chronometer-rates, we discontinued the chronographic apparatus beginning with the station Pachuca (Hidalgo), receiving and noting the radio signals by the method of coincidences by ear. This method has given splendid results with a smaller amount of computation. The additional apparatus needed for this method was a marine chronometer, which we regulated so as to make it gain one second in one sidereal minute. By means of this accelerated rate, we obtain as many as 12 coincidences in the interval of five minutes of mean time during which radio signals are sent out. The error with which a coincidence may be estimated is, at most, one second; this insures that the

error of comparison, when eight coincidences have been taken, shall be less than 0.02 second.

The introduction of short-wave signals, which began to be used not long after the world-longitude campaign, caused us to construct a new receiving-apparatus to catch the 37.4-meter signals from Arlington. These signals, which were used exclusively in our chronometer-comparisons at 10 p.m. (75th meridian time), have the advantage that they are received all over the country with great intensity, even when a small antenna is used. On the other hand, the signals sent by Arlington at noon with the above-mentioned wave-length are inaudible, and it would be desirable to have these signals on an even shorter wave-length, perhaps 20 or 25 meters. At the above-mentioned time we could catch only the 17,000-meter signals from Annapolis.

The Gravity Section was provided also, beginning at the station Pachuca, with a short-wave transmitter of variable power (7.5 to 50 watts) operated, according to circumstances, by dry-batteries or by the alternating current for city lighting. This apparatus made possible daily communication between the gravity-station in the field and the wireless station of the Central Office at Tacubaya, from which there could be received, not only all sorts of data and information, but all the signals from the Strasser clock, in case there was difficulty in receiving the standard-time radio signals. This occurred at stations Pachuca and Tiro Hermoso de Real del Monte. Whether because of the mineral character of the terrain, or because of the interference of high-tension lines, or because of the two things combined, the Annapolis signals were not received at Pachuca. In this case, the signals of the Strasser clock were transmitted from Tacubaya and received at Pachuca with great ease. In the interior of the mine, Tiro Hermoso, in which gravity was determined, no radio signal was received at any hour of the day or night. We explain this by the existence of a metallic screen, which might be the vein of mineral, intercepting the passage of the Hertzian waves. Since it was very difficult to lay electric lines in the shaft to bring to the station the signals received at a station outside, we installed a 50-watt transmitter at the mouth of the shaft and thus transmitted to the interior the breaks of a chronometer whose rate we verified outside the mine.

Both the chronometers and thermometers and likewise the auxiliary apparatus were those used in the 1925 work and need no special discussion.

In regard to the installation of the pendulum-apparatus at the various stations, we may say that, at most of them, the apparatus was mounted on a pier of concrete masonry, except at the station at the mouth of the Tiro Hermoso de Real del Monte, where use was made of an existing cement structure at the mouth of the mine, a circumstance that obliged the observer to work in an inconvenient position.

At all stations in masonry-rooms, we found no difficulty in keeping the temperature of the pendulums constant. On the other hand, when work is done in places with little shelter, as we had to do at the mouth of the Tiro Hermoso and lately at the camp at Tlamacas, at the foot of Popocatepetl, it is not possible to get the best results with the bronze pendulums. In such cases, what we did was to observe the temperature as often as possible in order to calculate the corrections with as accurate data as could be obtained.

Method of observation--At each station, observations were made with the three pendulums composing the apparatus, two days being assigned to the first pendulum and one day each to the other two, so that there are four 12-hour

periods of observation for the first pendulum and two for each of the others. We are accustomed to make two chronometer-comparisons a day, at noon and at 10 p.m., 75th meridian time, so that we obtained a series of observations completely independent of one another. We varied the order of the pendulums from one station to another.

The determination of the flexure of the support was made at each place by at least two observations during two or more different nights.

Coefficients of the pendulums--No new determination was made of the correction-coefficients of the three pendulums 9604, but the following values derived in 1922 by Mr. Ruiz were continued in use: Pressure-coefficient, x, 0.000000080 for all three; temperature-coefficient, y, 0.00000427 for pendulum A, 0.00000425 for pendulum B, and 0.00000426 for pendulum C; flexure-coefficient, z, 0.0000203 for all three.

These coefficients are for metric units, that is, for millimeters of mercury in pressure and for centigrade degrees for the temperature; for flexure the unit is one wave-length of sodium light.

Chronometer-rates--In the circuits of observations referred to above a radical change was made in the manner of using the radio time-signals for calculating chronometer-rates. It has been stated that this operation had previously been effected by recording the time-signals chronographically by the key-method. This method, which always gave good results, had the disadvantage that the reading of the tapes was laborious and, as a general rule, could not be done in the field with the greatest accuracy--a fact that prevented the preliminary field computation of gravity from being completed at the station itself and thus the observer did not have a safe criterion before leaving the station. To this disadvantage is to be added the uncertainty inherent in the method of recording by key, due to a possible change in the personal equation of the operator between successive operations.

Beginning with the gravity-station Pachuca, the method key-reading was replaced by the method of coincidences. To apply this method to the ordinary time-signals sent out on standard time by the Observatory of Tacubaya, or by the Naval Observatory of Washington, it was necessary to have a chronometer beating at a different rate from mean time. Accordingly, it seemed obvious to use a sidereal chronometer, with which one or two coincidences could be obtained within the interval of five minutes over which the signals extended; however, since two chronometers are used in the gravity-observations, one would have to be compared with the other, which could be done only with a chronograph. Moreover, what would usually happen would be that only one coincidence would be observed, and there is no doubt that the accuracy would be increased if a greater number of coincidences were obtained.

We obviated these two disadvantages by using a chronometer regulated to gain one second in each sidereal minute. This timekeeper we call the "vernier-chronometer." Because of its rapid rate about ten coincidences may be obtained in the five-minute interval of the radio signals and it lends itself also to the comparison of the chronometers by the same method of coincidences.

The sequence of operations is the following: Ten or twleve minutes before the beginning of the radio signals one of the chronometers used in the gravity-observations is compared with the vernier-chronometer. This comparison is made, beginning with an exact minute of the chronometer, by noting the

reading of the vernier-chronometer at this instant. From this instant on, attention is given to the ticks of the two chronometers to note their coincidences. The observer attends only to the readings of the vernier-chronometer and notes down the readings of the coincidences. The corresponding ones of the sidereal chronometer are found later by means of a graphic scale. Eight or ten coincidences suffice.

The operation is repeated with the second chronometer and ends in good time to receive the radio signals. It is very necessary, in receiving these signals, to note to the tenth of a second the readings of the vernier-chronometer at the beginning of each minute. These reference-data then serve to deduce the radio signals by which the coincidences are verified. The coincidences are noted that occur during the course of the signals (twelve in the most favorable case) always in units of the vernier-chronometer and, finally, another comparison of the chronometers with the vernier-chronometer is made. The whole operation can be performed in twenty minutes.

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GRAVITY-COMPARISONS IN EUROPE AND AMERICA

A. H. Miller

For the purpose of obtaining a direct gravity-connection between Ottawa and European base-stations the Dominion Observatory made, during the summer of 1928, relative determinations of gravity at Ottawa, Greenwich, and Potsdam. Ottawa is the base for some 100 field stations in different parts of Canada. Greenwich is a recognized base-station for gravity-work in Great Britain, India, and the Dominions. Potsdam is the base to which gravity-stations in all countries throughout the world are referred. Potsdam is also one of the limited number (possibly two) of places at which a strictly accurate absolute determination of gravity has been made. In addition to making possible a comparison between Canadian gravity-stations and other gravity-determinations throughout the world the work is of interest at the present time in view of its bearing on the question of the reality of the longitude-term in the formula for gravity. The value of the comparisons was increased by a connection that was made early this year between Ottawa and Washington. Potsdam, Greenwich, Ottawa, and Washington are therefore linked together by the observations.

Apparatus and adopted method--The Mendenhall apparatus belonging to the Observatory was used for the purpose. This consists of three bronze pendulums and two knife-edges. The observations at each station lasted for about two weeks, each pendulum being swung on each knife-edge for at least two days. The individual swings of the pendulums lasted for twelve hours. At Ottawa and Greenwich the standard sidereal clocks of the observatories were used to rate the pendulums. At Potsdam the standard Riefler sidereal of the Geodetic Institute was used and at Washington the time-signals from the Naval Observatory over the Western Union were used to obtain the chronometer errors. Otherwise the observations were carried out in the usual manner.

The pendulums were standardized at Ottawa in June 1928. The determinations were made at Greenwich and Potsdam in August and September of the same year. On the return from Europe the pendulums were again standardized in Ottawa during January 1929. Following this they were taken to Washington and

STATION	Knife Edge I: Periods of pendulums in seconds 1	2	3	Knife Edge II: Periods of pendulums in seconds 1	2	3	Weighted mean
June, 1928 Ottawa	.5011 3454	.5011 4541	.5011 4585	.5011 4587	.5011 4584	.5011 4618	.5011 4569
January, 1929 Ottawa	.5011 3415	.5011 4520	.5011 4574	.5011 4561	.5011 4567	.5011 4553	.5011 4561	980.6214
April, 1929 Ottawa	.5011 3416	.5011 4589	.5011 4576	.5011 4564	.5011 4563	.5011 4541	.5011 4563	
Sept., 1928 Potsdam	.5011 2418	.5011 2870	.5011 2846	.5011 2810	.5011 2819	.5011 2827	.5011 2795	981.2750
August, 1928 Greenwich	.5011 1871	.5011 2085	.5011 2048	.5011 1951	.5011 1943	.5011 1927	.5011 1916	981.1885
March, 1929 Washington	.5011 1713	.5011 2116	.5011 2068	.5011 1938	.5011 1932	.5011 1884	.5011 1924	980.1170

STATION	Knife Edge I: Value of g 1	2	3	Knife Edge II: Value of g 1	2	3	Weighted mean
Potsdam							981.2750
Greenwich	981.1877	981.1948	981.1920	981.1840	981.1832	981.1818	981.1885
Ottawa	980.6188	980.6234	980.6208	980.6175	980.6164	980.6128	980.6214
Washington	980.1159	980.1204	980.1178	980.1128	980.1123	980.1081	980.1170

Fig. 1--Table summarising results of standardizations of pendulums used in Canadian survey

on completing the determination there in March 1929 they were again standardized in Ottawa in April for the third time.

The results of the observations are shown in the table of Figure 1. At each station we have six determinations, one for each of the pendulums on each knife-edge. The values of g are expressed in the C.G.S. system and have been computed to the fourth place of decimals. The results are not as yet altogether final as it is desirable that a careful determination of the temperature-coefficients of the pendulums should be made. Observations for this purpose are not yet completed. For the purpose of this paper the value 41.9 has been adopted for the temperature-coefficient. This is the value obtained for the Coast and Geodetic Survey pendulums which are made of the same material. It is known that it is not very different for the Observatory pendulums and there is good reason for supposing that the results given here will not be materially affected when the final temperature-coefficients are determined and applied to these observations.

The value for Washington, 980.117, has been obtained by first determining Ottawa (using the June and January observations) and then determining Washington by reference to Ottawa. If the Ottawa observations are disregarded entirely and one considers the periods of the pendulums at only Potsdam and Washington the value for Washington comes out to be 980.118.

Comparisons with other determinations--The value for Greenwich agrees almost exactly with Borrass' adjusted value for Greenwich and is also in close agreement with Putnam's value for this station. The Washington value is, however, 0.006 cm per second per second greater than that obtained by Putnam. Ottawa and Washington were previously connected with observations by McDiarmid and the writer. The adopted difference between Washington and Ottawa resulting from their observations was 0.5080 cm per second per second; the difference indicated by the latest determination is 0.5044, which therefore agrees with the former adopted difference to within less than two units in the third decimal place for g (a convenient unit unfortunately without an English name, for which the millidyne has been suggested and for which the term milligal in recognition of Galileo is already adopted by German geophysicists).

The observations at Potsdam were taken on a low pier in the Mittelkeller, one floor below the room in which the absolute measurement was made some years ago. At Greenwich the apparatus was set up on a concrete pier in the new magnetic pavilion. The pier is about 350 yards east of Putnam's station in the Royal Observatory and about one meter below it in elevation, the height of the pier in the magnetic pavilion being roughly 46 meters above mean sea-level. At Ottawa and Washington the determinations were made on the gravity-piers in the basements of the Dominion Observatory and the Coast and Geodetic Survey, respectively.

The taking of the observations at the three stations outside of Canada was made a very pleasant task by the kindness of the directors and staffs of the institutions at which the observations were made.

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THE MEASUREMENT OF GRAVITY AT SEA

F. E. Wright

(Abstract)

Many attempts have been made to devise a satisfactory method for the measurement of gravity over ocean-areas. These methods have been chiefly of the elastic-deformation type in which the weight of a mass has been balanced either by an elastic solid or by a gas. None of these methods has thus far proved satisfactory either because of elastic hysteresis, or because of inadequate temperature-control, or of other defects in the apparatus. Hecker sought to measure gravity by another method, namely, by comparing the height of the mercury-column in a barometer with the barometric pressure determined from the boiling-point of water; but this method failed to give results of the desired accuracy.

Recently Dr. F. A. Vening Meinesz of Holland has developed a pendulum-apparatus suitable for use on board a submerged submarine or on a ship in calm weather. With the new instrument he has occupied nearly 200 stations at sea in different parts of the world and has demonstrated that it answers the purpose well.

The pendulum-apparatus consists essentially of three nearly isochronous pendulums mounted on a single supporting bar and swinging in a vertical plane crosswise to the length of the ship. The pendulums, a, b, c, are used in two pairs, a and b, and b and c; by means of a mirror-system the differences in phase between the pendulums of each pair are recorded photographically. Each pendulum pair behaves as a simple, fictitious pendulum, isochronous with the single pendulum and unaffected by the horizontal accelerations due to the movements of the ship. The pendulum apparatus is mounted in gimbals. The effects due to the roll and pitch of the ship are recorded by means of two auxiliary damped pendulums. The vertical accelerations are shown by the fluctuations of the phase-lag curve due to the time-breaks from the sidereal- and mean-time chronometers whose beats are not exactly isochronous with those of the fictitious pendulum. The total time required to occupy a station is half an hour.

From the photographic record thus obtained, together with measurements of

the chronometer daily rates, of the temperature, barometric pressure, humidity, latitude and longitude of the ship's position, its course and speed, direction and velocity of the ocean-current, and depth of submergence, it is possible, by a routine method of measurement and computation to determine the value of gravity with an accuracy of approximately one part in 250,000.

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RECENT DEVELOPMENTS IN TIME-SERVICE METHODS

C. B. Watts

The Naval Observatory time-service has undergone in recent years an interesting development and expansion. This has been brought about by a demand for increased accuracy in the time-signals. The task of sending regular signals of sufficient accuracy to meet the needs of navigation and every-day life is, of course, relatively simple. When, however, the signals are to be used by longitude-parties and radio laboratories and are to be subjected to comparison with similar signals emanating from foreign observatories, the highest obtainable degree of accuracy becomes desirable.

It was 16 years ago that the radio station at Arlington was completed and began broadcasting our time-signals. Up to that time the signals had been sent only at noon, and had been omitted on Sundays and holidays. They were transmitted chiefly by telegraph and the code used was little known except to jewelers and railroad men. With the completion of the Arlington station the Observatory began sending two signals daily, and the practice was started of publishing corrections to the signals as determined from later observations. Until comparatively recently the correction to a signal was determined by a comparison of the final dash of the signal with the standard clock. It has been found, however, that the signals from the break-circuit wheels of the standard clocks are not spaced with perfect uniformity. This condition is due to a peculiarity of the escapement used in these clocks and has no particular bearing on their timekeeping qualities. For this reason the practice has been adopted of comparing at least ten widely separated dashes of each time-signal with the standard clock and basing the published error of the signal on the mean.

The somewhat irregular spacing of the signals made by the standard clocks is unavoidable so long as the signals are taken from the break-circuit wheel. However, the signals can be taken directly from the pendulum by means of a photo-electric cell, and this method has been suggested and tried experimentally in various places.

One of the minor difficulties of the time-service is to keep the transmitting clocks perfectly in beat. Our code consists of sending a short dash for every second, except certain ones which are omitted for identification. The dashes for the even seconds are made when the pendulum is swinging in one direction and those for the odd seconds when it is swinging in the other; so that they are to a large extent independent of each other. If the clock is out of beat the odd-second dashes will divide the intervals between the even seconds unequally. This discrepancy is not apparent on an ordinary chronograph but it can be observed readily with more precise apparatus, such as an oscillograph. To compensate for it we measure an equal number of odd and even dashes, and recommend that the users of the signals do likewise. Two remedies have

been proposed for this effect. One is to discard the second pendulum and substitute a half-second pendulum, which would initiate only one signal during each complete oscillation. Another way, and one that would perhaps have other advantages, would be to alter the code so as to sound only the even seconds.

Such a serious change in the code would hardly be justified for this purpose. There is, however, a small change in the code under consideration at the present time. This is a plan to identify each minute of the signal by omitting a dash toward the end of the minute. The number of dashes immediately following the omitted one would represent the number of minutes still to elapse before the final long dash.

The excellent performance of the Shortt clocks at the Royal Observatory led the astronomers there to take a step in computing clock-rates which had not before been considered necessary. This involves the consideration that sidereal time, as ordinarily defined, does not progress with perfect uniformity. If we define sidereal time as the hour-angle of the true vernal-equinox, it is obvious that the variable portion of the precession, known as nutation, by causing the hour-angle of the equinox to vary somewhat irregularly, will produce corresponding inequalities in the progression of sidereal time. For this reason a clock being rated in sidereal time may be charged with small departures from a uniform rate which are apparent rather than real. By using the hour-angle of the mean equinox of date, which may be called mean-sidereal time, a somewhat steadier rate of the sidereal clock may be expected.

The practice of receiving and recording radio time-signals for the purpose of comparing time-determinations made in various parts of the world is becoming very common. At the Naval Observatory we record two foreign signals fairly regularly--that of the new station at Rugby at 1 p.m. and the Bordeaux signal at 3 a.m. Both of these stations use the revised international code, consisting of a dash at the beginning of each minute, and each minute divided into 61 parts (instead of 60, as we do it) so that comparisons by the method of coincidences can be made easily. This code is a marked improvement over the old rhythmic code, which had no definite time of beginning and ending, and was useless for 24 hours after it was received. Radio signals are now easily recorded automatically on a chronograph and the method of coincidences is not so widely used as it was once.

Just preceding the 3 a.m. Bordeaux signal we send one of our own which has recently been added to the regular program. This signal has been particularly useful to surveying parties in the far west, which are still in daylight in the summer when the 10 p.m. signal is transmitted.

The Observatory determines the corrections to as many of its own signals as possible. Arlington transmits on five frequencies, and three of these, together with the powerful signal from Annapolis, are recorded daily. The corrections to these signals have hitherto been sent out monthly to users of them, but it is now planned to mail them weekly.

Several distant radio stations are operated by the telegraphic signals sent over the Western Union wires. Of these stations the principal one is San Diego. For many years it transmitted the signals sent out by the chronometer-and time-station at Mare Island, but has recently been operated directly from Washington. There is no opportunity during the five-minutes duration of the signals to determine the correction for this station. Its signals are not therefore suitable for scientific purposes as the lag is large and uncertain. In case of trouble on the long circuit across the United States it would proba-

bly be operated by one of the Western Union clocks on the Pacific coast.

It is interesting to reflect, however, that one or more of the accurate signals from Annapolis and Arlington may be received in practically all parts of the world. From the simple beginning of the time-service in 1865, when the fire bells in the engine-houses in Washington were struck by signals from the old Observatory, the area served by its signals has expanded until surveyors in Africa and Australia can utilize them without difficulty.

The Observatory has engaged in considerable experimental work in attempts to improve the accuracy of the time-signals. One of my colleagues, Mr. Sollenberger, has been working on a precision clock with an electric-gravity drive and a slave-clock to initiate impulses. Various devices for comparing clocks with each other with a higher degree of accuracy than is attainable with an ordinary chronograph are under consideration. Such devices are already in existence, but for the most part are not well adapted to the needs of the time-service.

It appears that the element in a time-service most in need of improvement is the original determinations of time on which the signals are based. The probable error of a time-determination derived from an evening's work with a transit instrument is greater than it should be if we are to be sure of the hundredth of a second in the correction to a time-signal. The uncertainty of a time-determination is, of course, due to a number of causes, such as errors of star-places, lateral refraction, errors in instrumental constants, and difficulties peculiar to the method of observing. The traveling-wire micrometer is usually conceded to be the best means yet devised for making accurate time-determinations. Opinion is perhaps somewhat divided on the relative merits of driving the micrometer by hand and driving it with a motor. Some experiments still in progress have convinced me that a really efficient motor-drive leaves little to be desired.

The synchronous electric motor, which has proved to be so useful in multiplex telegraphy, television, and the like, is also of value here. A small synchronous motor, weighing only a few ounces, has been mounted on the micrometer of one of the transit instruments and geared directly to the screw. The position of the traveling wire at any instant is adjusted by the observer by regulating the frequency of the current driving the motor. The preliminary results indicate that a material increase in the accuracy of the observations can be obtained by this means.

The time-service of the Naval Observatory, reaching as it does into all parts of the world and many phases of life, frequently receives suggestions and constructive criticisms which are of value. It is hoped that the friendly cooperation of other scientific agencies will continue. Meanwhile the time-service will endeavor to improve its methods and increase the availability of its signals and otherwise comply with the spirit of the word "service."

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RECENT DEVELOPMENTS IN GEODETIC INSTRUMENTS

D. L. Parkhurst

During the past two years there have been a number of improvements and new instruments developed by the United States Coast and Geodetic Survey in its geodetic instrumental equipment. The principal matters to be mentioned are the completion of a first-order theodolite, the development of a second-order instrument of the same nature, and the improvement in the design and method of graduation of the precise level-rod.

The first-order theodolite was described before this Union two years ago (Bull. Nation. Res. Council, No. 56, 46-49, 1926) at which time it was under development. It has since then been completed, has had a season's service in the field, and has proven quite satisfactory. This instrument is compact and light, as such instruments go, as it has an overall diameter of less than 10 inches and weighs but 33 pounds. Every effort was made in its design to facilitate adjustment and repair under field conditions, as it is the belief of the engineers of the Coast and Geodetic Survey that it is better to assume that any instrument will eventually become out of adjustment and that in that case it should be readily brought back into adjustment by the observer in the field.

A number of theodolites used by the Survey in the past have been susceptible to variations in temperature, particularly at the vertical axis. An adjustable thrust-bearing has usually been provided to take care of this. There is a certain disadvantage in this, for if this thrust-bearing is set up too far, it will introduce play at this point, with consequent error in the reading. In the new instrument, use is made of a vertical axis whose bearing is in the form of two cones of widely dissimilar angles yet whose vertices are common. In this design, regardless of the temperature-coefficients of the materials of the axis and socket, expansion laterally and longitudinally is proportional and the fit is maintained under all conditions. This has been amply proven by placing the instrument in a temperature-chamber and varying the temperature from zero to 125° Fahrenheit, which is a greater range than would be customarily encountered in the field. No perceptible change in it or friction could be noticed in this test. In the second-order instrument this same principle of design is incorporated.

We have also introduced a ball-bearing in the alidade-clamp, as we found that fully 50 per cent of the total friction of rotation of the alidade could be found at this point, even in the most carefully manufactured clamp. The special ball thrust-bearing which has been introduced has practically eliminated all friction at this point. The clamp itself is a sort of brake-shoe, which is pressed against its seat by a pointed thumb-screw and locks the alidade with extreme rigidity with a very light touch of the thumb-wheel. On the other hand, when the thumb-screw is reversed the pressure on the block is not merely released but, by means of a collar, the block is absolutely withdrawn from the braking surface.

The most sensitive part of a theodolite whose circle is read by micrometer-microscope is the micrometer-box itself, and what we believe to be a most important change has been made in the design of this element. The customary micrometer consists of a sliding block, usually rectangular in shape, which may be moved by means of a fine pitched screw, reaction being taken up by a spring or springs, which are preferably under tension rather than compression. Although the forces of friction are very slight, yet the actuating forces are proportionately slight, so that the amount of friction at this point is most

decidedly of moment. Furthermore, the greatest care and skill must be exercised in the manufacture of the micrometer-box in order to obtain the practically perfect fit that is necessary. Ordinarily almost no adjustment is possible.

The new design of micrometer-box, which is being incorporated into the Survey's second-order theodolite, has the block or runner of the micrometer made of hard steel. Two V-shaped grooves extending longitudinally along either side act as race-ways for four hardened steel balls, which also run in similar grooved pieces of steel set into the body of the micrometer-box. One of these race-ways is fixed, the other is adjustable. The runner operates very nearly without friction and, as far as laboratory tests have been able to determine, the runner reverses absolutely with the reversal of the screw, which will mean that in observing it will not be necessary to make readings using one direction of the runner only. It may be used either way, either with or against the spring-action. The adjustable feature of the race-way makes it somewhat easier and simpler to construct the micrometer-box and permits the taking up of wear. The freedom of motion of the runner allows us to use a single and very light tension-spring.

As this instrument will be used at night the drum of the micrometer is made of a ring of milk-colored glass with the lines and figures etched upon the outer surface, and blackened. A shielded electric bulb illuminates the interior of this ring, the light being diffused by the milky pigment in the glass and the drum is much easier to read than the usual metallic type which is ordinarily illuminated by a flashlight. With the latter method the reflected light makes it difficult to read the graduations on the drum.

An improvement has been made in the method of graduating the geodetic level-rods, and also in the design of the rod itself. This rod consists of a strip of invar rigidly attached to a hardened foot-piece of steel, the whole being supported by a wooden rod. The invar is sprayed with several coats of a white pyroxylin lacquer and the black checker work is applied in a similar manner by means of a mask and spray gun, a special machine having been built for the purpose. A recent improvement in the design of the rod is to make the face concave, so that the invar strip will be approximately a quarter of an inch below the top surface of the wood, an arrangement which protects the graduations from being worn by handling and from being scratched.

The machine for applying the graduations was designed and built by the Survey and makes use of the comparison-method; an invar bar of the same characteristics as the invar strip, accurately ruled into centimeter-divisions and calibrated, is used with the machine. The carriage is actuated by a long lead-screw and carries a turreting mask having slots one centimeter wide. The carriage is moved along by the lead-screw until the parallel wires of the microscope at the rear of the carriage straddle one of the division-lines of the comparison-bar. The mask is then lowered by a cam against the work and the exposed area sprayed with a quick-drying lacquer. The mask is then lifted and traversed to the next division. This same machine is used for laying off the calibration-lines at each meter-interval, the rod being calibrated at the 0.2, 1.2, 2.2, etc. spaces. By means of a solid tee-square and a scriber which runs in a groove built into the tee-square these calibration-lines may be read and ruled with great accuracy, the tee-square having an observing microscope similar to that of the carriage. By means of this arrangement we have been able to produce rods whose index-correction from the foot to the 0.2-meter space is practically zero, and all rods are practically identical so that there is no longer so great a necessity for the pairing of the rods. Furthermore, the laying off of the meter-divisions can now be performed with

so much greater accuracy and uniformity that we have been able to increase the accuracy of calibration from one-tenth mm to one-hundredth mm.

This increase in accuracy is reflected in the accuracy of the leveling, and the Bureau is now experimenting with a new method of conducting its leveling whereby the rear rod-man will not advance to the front each time the instrument's position is changed. It has been the practice heretofore for the rear rod-man to proceed ahead of the instrument after each observation, which means that he has to go twice as far as the level-man, and usually the instrument-man will have his instrument set up before the rod-man is in position. Under the new system both rods and instrument will advance the same distance for a certain number of set-ups and there will be greater rapidity in performing the work, without in any way sacrificing the accuracy. At present we are having the rear rod-man advance to the fore after a number of positions, but it is to be hoped that we will eventually find even this to be unnecessary.

U. S. Coast and Geodetic Survey,
Washington, D. C.

PROGRESS OF GEODETIC WORK IN CANADA DURING 1928

Noel Ogilvie

Ottawa, Ontario, "the Washington of the North," being the seat of the Federal Government, is the headquarters of the Geodetic Survey of Canada and is situated at the point where the Rideau River joins the Ottawa; a very picturesque sight both from a natural and artistic standpoint.

In 1903 Major E. H. Hills, C.M.G., made a report on the Surveys of Canada indicating how a geodetic survey of the Dominion of Canada should be carried on.

It will be seen by a comparison of the map of Canada showing the triangulation finished (Fig. 1) and projected up to the end of 1928, with the map of Canada showing Major Hills' projected triangulation (Fig. 2), that the suggested program is being followed in the main and that any changes have been either elaborations of his scheme, as the uses of the triangulation have become more appreciated, or have been necessitated by the development of the country along lines not discernible in 1903 when the report was issued.

Every effort is being made to extend geodetic work into the great mining areas in the northern parts of Canada.

During the field season of 1928, although the weather was unfavorable, the number of triangulation-stations completed was considerably greater than in other years due largely to the use of the Wild theodolites.

In New Brunswick the Matapedia Valley net which was started from the basic triangulation along the St. Lawrence in 1927 was completed in 1928 southerly through the Matapedia Valley to the Bay of Chaleur net. Most of the net was also completed in 1928 across the northerly and westerly portion of New Brunswick to meet the triangulation-scheme which had progressed northwards from Passamaquoddy Bay. These two nets will be augmented by others in the United States which subdivide the large 1,800-mile circuit of triangulation which was closed in 1925 composed of nets in the eastern sections of both countries. The above nets are about 225 miles in length and a few weeks of the

1929 season will see the completion of this work.

In the western part of the province of Quebec a five- or six-year program was commenced in 1926 composed of a 500-mile east-and-west net along the most northerly railway in that area, together with a 200-mile net northward from Ottawa to meet the above east-and-west net. The first of these schemes completes a 1,000-mile circuit and the latter subdivides this loop in two. Angular measurements on about 135 miles of the east-and-west net were completed in 1928 and 215 miles still remain to be done. Only the reconnaissance has been done on the north-and-south net. In the Rouyn mining area at the west end progress was very satisfactory in 1928 due largely to the use of daylight signals and the Wild theodolites. The remaining portion of the above program lies in rough, little organized country in which progress is necessarily rather slow. The reconnaissance, which is now practically complete, was speeded up in two ways. A large industrial company which was interested not only in the mapping of the area, but also realized that the location of the triangulation-stations would provide valuable data as to the choice of sites for fire-detection lookout-towers, offered its cooperation. The use of an airplane during this last winter also speeded up this work.

Eighty-foot steel towers for fire-detection purposes are being built at most of the triangulation-stations selected on the net northward from Ottawa and the trails and telephone-lines, which are a necessary complement of the

fire-detection system, will facilitate the angular measurements to a great extent.

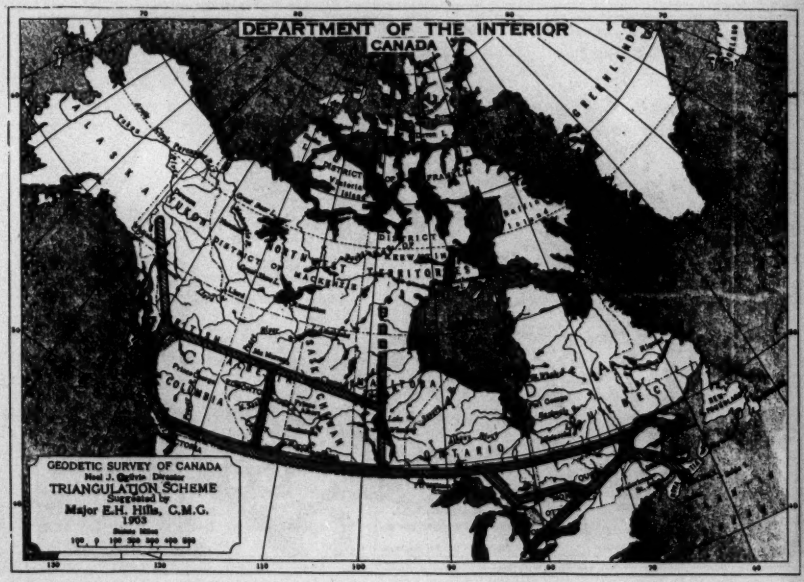
In Quebec, one base was established near Senneterre.

The main triangulation-program in the province of Ontario in 1928 was confined to the mineralized areas of northern Ontario from Kirkland Lake area north to Lake Abitibi and west to the Kamiskotia district, also in the Sudbury nickel area.

The northern part of the province situated in a clay-belt is being given geodetic control by means of a precise traverse which starts from the end of our triangulation near Iroquois Falls and follows in a northwesterly direction along the T. and N.O. railway to Cochrane. This country is very flat, and it is proposed to continue precise traverse a distance of about 270 miles.

A primary scheme was laid out westerly connecting with the Ottawa River net just west of Mattawa extending across Lake Nipissing to Sudbury, thence to the International Boundary at Sault Ste. Marie.

The above control serves both the provincial and Topographical Survey interests for mapping purposes in a highly mineralized country.



The projected triangulation in the Prairie Provinces includes a net northward from the International Boundary through Calgary to Edmonton. The north end of this net will eventually be joined by way of the Yellowhead Pass to the corresponding British Columbia net while east from Edmonton the net will ultimately extend to the north of Lake Winnipeg and east across northern Ontario. Branches from this net are required to control the Flin Flon and other mining areas of northern Saskatchewan and Manitoba, also Fort Churchill on Hudson Bay and the region southward to the International Boundary. About 600 miles of this large program has been completed, about 250 miles having been finished in 1928.

The program in the northern part of British Columbia is a continuation of the 1,300-mile precise loop which has been projected. Of this loop about 800 miles runs through the interior of the province starting from Prince Rupert via Prince George and Ashcroft to Vancouver (Fig. 3). From Prince Rubert to Smithers transportation and other difficulties dictated that the method of precise traverse be used, but easterly from Smithers triangulation was resumed. At the close of the 1928 season angular measurements had reached Prince George 400 miles from the coast. About 200 miles were finished in 1928. The executing of this primary triangulation and its extension southerly will serve the needs of the provincial authorities for mapping and subsidiary triangulation.

On Vancouver Island the angular measurements were completed of a secondary triangulation-net extending from the primary scheme in the Strait of Georgia

across the Island by way of Alberni and Barkley Sound to the west coast. This work was undertaken to furnish control on the west coast of the Island for the federal hydrographic survey and provincial land-surveys.

In certain types of country-triangulation reconnaissance can be carried on by airplane with wonderful speed and great economy. In 1921 an officer of the Geodetic Survey of Canada made a successful triangulation-reconnaissance by airplane in the mountainous district northeast of Vancouver, and while the advantages of the method in mountainous country have been known for several years, its application to flatter country has only recently been realized.

During February and March 1929 two experimental airplane-flights were undertaken by the Geodetic Survey of Canada in cooperation with the Royal Canadian Air Force to determine the suitability of the airplane for triangulation-reconnaissance; the transportation of triangulation-parties both for station preparation and observation and for preliminary reconnaissance for triangulation routes in out-of-the-way places.

In a total absence from Ottawa of ten days time 27 stations in two triangulation-nets of 190 miles in length and an area of 4,500 square miles were selected in a country which could not have been covered in less than two seasons by the conventional methods. The planes belonged to the Royal Canadian Air Force and were not in use at the time, so that the cost was small, but even estimating the flying time at the highest commercial rates, the cost of the work, including salaries, was only half what it would have been by the ordinary methods.

The two areas selected for the experimental airplane-reconnaissance were about 250 miles north of Ottawa, and their axial lengths were 75 miles and 115 miles, respectively, in an east-west direction. The hills were from 200 to 1,000 feet above the lowlands and transportation in summer was almost entirely by canoe along the many rivers and lakes or by back-packing.

A Fairchild Wasp cabin-monoplane with a single 400-horsepower engine and equipped with skiis was used and proved very satisfactory.

The instrumental equipment consisted of a small stand at one side of the top of which a good compass was mounted and on the other side of the top of the stand there was a 16-inch circular revolving board. Around the edge of the board was a protractor and on the board a map was oriented. A glance at the compass and a turn of the board sufficed to orient the map, and an open-sight alidade completed a satisfactory plane-table outfit.

Three flights over an area were usually found necessary. The route of the first flight was selected where it was safe to fly as low as possible--so that the hills would show up plainly. The prominent hills were approximately plotted by intersection from different points and likely lines of sight were indicated on the map.

A second flight was made after this data had been digested and a tentative scheme laid down. Each hill was flown over and located more accurately with reference to the topography, and flights were made as low as the tops of the hills wherever safe, to test the intervisibility of lines.

Following this flight there were usually a few doubtful questions which, if possible, were cleared up on the third flight. Some of these could only be removed by visiting certain stations, but this was not done on the experimental

flights. Where doubt existed, as to the intervisibility on a diagonal of a quadrilateral, an alternative scheme was selected. On the third flight photographs of the hills and routes of approach were also taken.

Where it was not safe to fly low, over or around a hill, side views of the lines from a low elevation over a lake were found very useful.

A general impression of the test is that the applicability of the method to various types of country depends on the safe height at which the plane can be flown. This depends entirely on the dependability of the plane or on the availability of landing places. With a trimotored plane no difficulty should be experienced in having safe, satisfactory, economical, and exceedingly quick reconnaissance performed in nearly all types of country. A few of the stations would need to be visited where lines of sight were doubtful.

Other impressions of these preliminary tests are that the magnetic compass needs watching, that the method trains a man to think very quickly, that it is especially applicable to country where transportation is slow and difficult; also that sea-sickness is not confined to the sea.

The rigorous adjustment of the continuous chain of triangulation-nets in eastern Canada, along the St. Lawrence River, Gaspe and Bay of Chaleur, New Brunswick and Nova Scotia, has advanced very satisfactorily.

There have also been completed the adjustments of two nets--one across Vancouver Island and the other along the southwest coast of Nova Scotia. The information derived from the adjustment of the first-mentioned net was requested by the Department of Lands of British Columbia. In addition to the work above mentioned, satisfactory progress has been made on the nets in northern Ontario, Quebec, and also on the precise traverse in British Columbia.

The above work has been greatly facilitated by the use of especially constructed nomographic charts which reduce the work of deriving the final coordinates by about 70 per cent. It is hoped to incorporate this principle in a separate pamphlet as it is felt that it would assist other survey-divisions in work of a similar nature.

A publication entitled "Adjustment of primary triangulation in western Ontario" is now being prepared and will shortly be published.

Considerable progress has been made along the lines of mathematical research and adjustments. A publication is being prepared, and will shortly be published dealing with "Simultaneous adjustment of precise traverse and triangulation and differential adjustment of observations."

After an extensive investigation into a new problem which arose as to how to incorporate in one adjustment both precise-traverse and triangulation-data, a special theory has been evolved and will be published as "Simultaneous adjustment of precise traverse and triangulation" and it is hoped that this theory will prove very helpful. Its application in Canada will be chiefly in British Columbia and northern Ontario where stretches of triangulation are joined by traverses.

There has been great demand for Publication No. 11, "Geodesy," which was distributed during the past six months.

In the southern part of the province of Ontario secondary leveling was

carried along the "Blue-Water Highway" starting at Sarnia and continuing northerly to Midland, while along the Ottawa River another secondary line of levels was carried from Hull to Chapeau, Quebec, following the Canadian Pacific Railway to Waltham and thence the Hull-Chapeau Highway, a branch of this line crossing the Ottawa River at Portage-du-Fort and continuing to Pembroke and Golden Lake.

In British Columbia the levels along the Pacific Great Eastern Railway were extended to the end of track at Quesnel. The work on the prairies consisted in the construction of 15 monuments of the type designated as fundamental bench-marks. These were built in certain selected cities and towns in Alberta and Saskatchewan. At the present time 67 of these fundamental bench-marks have been established.

A special inspection party consisting of an engineer and one railway employee spent the whole field season in bench-mark inspection. In all the total of 2,267 bench-marks were inspected of which 11 per cent were destroyed or could not be found by the inspector.

A series of precise-leveling publications containing the first adjusted elevations in Canada will be printed within the next three months.

A publication entitled "The adjustment of the precise level-net of Canada" will shortly be published. This shows the methods of adjustment used; the meaning of the various adjustments and the effects of each; the datums on which all adjustments are based; the precisions arrived at, and the resulting adjusted elevations of all junction bench-marks.

As indicated in a previous address (Bull. Nation. Res. Council, No. 61, 44-50, 1927) before the Section of Geodesy, the study of isostasy has been engaging the attention of the Geodetic Survey of Canada.

For the past three years one field party has been especially engaged in making longitude- and latitude-observations at geodetic triangulation-stations to determine the deflection of the vertical. There are now available figures for the deflection of the vertical at more than 100 stations across Canada. The corrections for topography are now being deduced and also the corrections for the isostatic compensation.

Within a short time it is hoped that there will be available a good deal of information concerning the isostatic condition of the northern part of the North American continent. The field staff on this has lately been increased in number. Instead of one party there will be two wholly engaged in deflection of the vertical determinations.

Geodetic Survey of Canada,
Ottawa, Canada.

GEODETIC COMPUTATIONS AND INVESTIGATIONS

Henry G. Avers

Since last year's meeting of the Section the usual computations have been carried forward by the United States Coast and Geodetic Survey in triangulation, leveling, geodetic astronomy, and gravity.

The computation and adjustment of the results of triangulation form by far the greater part of the computations that have been made. This is true not only because more field observations have been made in this class of work but also because the computations necessary to reduce the observations are more extensive and the adjustments require the solution of large sets of equations to make the work consistent within itself and with the existing triangulation to which it connects.

Most of the first-order arcs of triangulation that have been adjusted to the triangulation-net in the past have required, on the average, about 100 equations. Two arcs in southwestern United States, forming a cross and adjusted to the net in a single piece, required the solution of 345 normal equations. This is the largest set of equations ever solved by the United States Coast and Geodetic Survey and it took one man 82 days to make the forward-solution.

The adjustment of the triangulation west of the 98th meridian, which was reported upon at previous meetings of this Section, is practically completed. The geographic positions of all the stations in the main arcs of triangulation have been computed and there remain only a few supplementary stations for which final values are yet to be determined. A report will be published giving the details of the adjustment and a complete description of the method used.

The computation and adjustment of all the first-order triangulation along the northwestern coast of North America between Puget Sound and Skagway have been completed. This triangulation is composed of the Canadian arc extending from Puget Sound to Dixon Entrance and of the American arc extending from Dixon Entrance to Skagway. The adjustment of the Canadian arc was made by the Geodetic Survey of Canada and it is based upon the geographic positions of points in the vicinity of Puget Sound which had been fixed by the adjustment of the triangulation west of the 98th meridian.

The American arc was adjusted to the northern line of the Canadian arc and so provides geographic positions based upon the North American Datum 1927 for all the triangulation in southeastern Alaska comprising some 9,000 points. The geographic positions and the descriptions of the 220 stations of the first-order work in southeastern Alaska are being prepared for publication (W. F. Reynolds, First-order triangulation in southeast Alaska, Dept. Comm., U.S. Coast Geod. Surv., Spec. Pub. 164, 157 pp., 1930).

Preparations are being made for the adjustment of the triangulation east of the 98th meridian by the same method that was used so successfully by the Coast and Geodetic Survey in the adjustment of the triangulation west of that meridian. The field work for a number of the arcs that are to be included in this adjustment has not yet been done. The computations can proceed, however, since each of the arcs of triangulation between the junction-points forms an individual computation preparatory to fixing the geographic positions of the junction-points.

An adjustment has been made of the triangulation covering the Hawaiian Islands. The field observations for this work were begun in 1870 and continued intermittently until their completion in 1928. The triangulation of the various islands forms a continuous scheme through the group and it is adjusted to a common datum. A complete report on the adjustment has been prepared (H. C. Mitchell, Triangulation in Hawaii, Dept. Comm., U.S. Coast Geod. Surv., Spec. Pub. 156, 240 pp., 1930). Geographic positions are given for more than 1,300 triangulation stations of which nearly 700 are described.

At the request of the United States Navy, the Coast and Geodetic Survey made the isostatic reductions for the 49 gravity-stations observed by Ir. Meinesz of the Dutch Geodetic Commission while on board the U.S. Submarine S-21. These gravity-observations were made over the Sigsbee, Bartlett, and Nares deeps in the vicinity of the West Indies, and along the Atlantic Coast south of Chesapeake Bay.

The investigation of the slope of the mean sea-level surface has been extended to include the combined level-nets of Canada and the United States. The first investigation included the leveling in the United States only and it was found that there is a downward slope in the mean sea-level surface of the Gulf of Mexico from west to east and an upward slope in the mean sea-level surface of the Atlantic and Pacific oceans from south to north.

For the present investigation an adjustment is being made of the combined level-nets involving about 75,000 miles of leveling which form 270 circuits. These nets are connected with the plane of mean sea-level at 25 places.

In the adjustment only the sea-level connection at Galveston, Texas, is being held fixed. The results will give the elevation of the mean sea-level surface at the other tidal stations with respect to it. While the adjustment is being made primarily for this investigation the results will also probably be used as a working basis for assigning elevations in the bench-marks common to the two nets along the international boundary between the United States and Canada.

U. S. Coast and Geodetic Survey,
Washington, D. C.

ACCOMPLISHMENTS IN FIELD GEODESY DURING THE YEAR APRIL 1928 TO APRIL 1929

William Bowie

With few exceptions the field geodetic work in the United States is carried on by the United States Coast and Geodetic Survey. This has been the case for a number of years, especially since the Survey has been able to carry on all of the work for which it has been given funds. Of course, the operations of the Survey could be greatly extended if the government officials should feel that greater activity in field geodesy were desirable.

A few of the cities are now carrying on geodetic operations consisting of triangulation and leveling for the control of surveying, mapping, and for other engineering purposes, but from a scientific standpoint the results of the city geodetic work are of small importance.

The United States Geological Survey, in connection with its topographic surveying, executes what is called third-order traverse and leveling for the

immediate use of topographic engineers. The Corps of Engineers of the Army and its various commissions are executing triangulation, leveling, and traverse of about third order, in connection with their river and harbor improvements and in the control of the Mississippi River.

These operations are practically all that are carried on in this country that can be classed as geodetic outside of the work done by the Coast and Geodetic Survey. The geodetic operations which are of interest to the members of the American Geophysical Union are those which have a scientific bearing and the results of which may be used in throwing more light on the physics of the Earth.

The principal work of the Coast and Geodetic Survey along geodetic lines consists of the extension over our three million square miles of area of a network of first-order triangulation, to which will be added a number of arcs of second-order work. The combined first- and second-order nets will, when completed, supply triangulation data within 25 miles of any place in the country. Triangulation in Alaska was begun by the Coast and Geodetic Survey, but it was discontinued a few years ago and has not been resumed. Alaska has an area of more than half a million square miles and it would seem to be desirable to have arcs of first-order triangulation extended along the principal routes of travel in that Territory in order that cadastral, topographic, mineral, and other surveys and resulting maps might be properly coordinated. No doubt this is something that will be given favorable consideration by government officials and we may look forward to greater activity in the Alaskan area in the years to come.

The Coast and Geodetic Survey is extending a network of lines of first-order leveling over the United States and to those lines will be added lines of second-order leveling. Eventually, when the fundamental leveling-net has been completed, there will be no place in the country more than about 25 miles from a bench-mark of first- or second-order leveling.

In connection with the triangulation, the Survey makes astronomic observations for the determination of the latitude and longitude of certain triangulation-stations and the azimuths of some of the triangle-sides. In connection with triangulation, base-lines are measured to furnish the lengths used in computing the triangulation across country.

The Coast and Geodetic Survey has, as one of its major activities, the determination of the value of gravity at points in the continental United States, territories, outlying possessions, and along coasts of islands contiguous to our area. One other line of activity is making the observations for the determination of the variation of latitude. Only one station is involved in this work, and that is the one at Ukiah, California.

The work done during the last year by the Coast and Geodetic Survey in the field of gravity was supplemented by the determination of the values of gravity aboard the Submarine S-21 of the United States Navy by Dr. F. A. Vening Meinesz, in the Gulf of Mexico, the Caribbean Sea, and in the waters off the southeastern coast of the United States.

The work accomplished during the past year was designed to fit into the general schemes of the several classes of geodetic work.

Triangulation was executed in the states of Iowa, Missouri, Arkansas, Kentucky, Ohio, Pennsylvania, Maine, and California. Preliminary or

reconnaissance-surveys for triangulation were made in the states of New York, Pennsylvania, New Jersey, Arkansas, Texas, Louisiana, Mississippi, Alabama, Florida, and Georgia. During the next year it is expected that the measurement of angles will be made along the arcs which were located by this preliminary reconnaissance.

All of the triangulation, except that in California, was designed to fill in the first-order triangulation-net in the eastern half of the country in order that a readjustment of this net may be made along the same lines that were followed in readjusting the western half of the net. It is expected that the remaining arcs in the eastern part of the country needed in this net-adjustment will be finished by the autumn of 1930. The triangulation in California was part of a general plan which had been formed by the Coast and Geodetic Survey in cooperation with the Advisory Committee in Seismology of the Carnegie Institution of Washington. It consists of an arc of first-order triangulation running from the vicinity of Newport Beach to Lucerne Valley. That arc crosses a number of fault-zones which have been active during recent times--geologically speaking. Many supplementary stations were established along this first-order arc and tied into the first-order stations. These points were placed close together in or near the fault-zones. Should an earthquake occur along any of the zones, the amount of horizontal movement of the triangulation-stations established on or near the fault can be determined by reobserving the angles of the triangles involving these supplemental stations.

The selection of stations has been begun for an arc of triangulation that will extend from New Orleans, Louisiana, northward along the Mississippi River to Cairo, Illinois. This work is being done by the Coast and Geodetic Survey in cooperation with the Mississippi River Commission. It is expected that this work will be completed by the end of 1930. It will fit into the eastern half of the triangulation-net of the country and will furnish final positions for all of the traverse- and triangulation-stations which have previously been established along the River by the Commission.

An item of special interest from the scientific standpoint was the determination by first-order leveling of the elevation of Mt. Whitney, in southern California. As far as is known, this is the second very high peak in the United States to which spirit-leveling has been carried. The other is Pike's Peak. The leveling up Mt. Whitney started from bench-marks in Owens Valley along the railroad in the vicinity of Lone Pine. That leveling, in turn, has been connected with mean sea-level through a net of leveling at San Diego, San Pedro, and San Francisco. The elevation of Mt. Whitney, referred to sea-level, is 14,496 feet. This is slightly lower than the elevation determined by the leveling of a lower order executed by the United States Geological Survey engineers some years ago. The difference of 5 feet may be accounted for by the errors of observation and of the instrumental errors involved in the work of the Geological Survey. Of course, there is a possibility that some part of the difference may have been due to actual earth-movements but there is nothing to prove that this is the case. It would be interesting, however, if, ten or fifteen years from now, a line of levels from Lone Pine to the top of Mt. Whitney could be rerun and the elevations of all bench-marks redetermined. From such work it might be possible to secure evidence as to whether or not the great mountain-mass under Mt. Whitney Peak is rising or sinking, or is remaining stationary with respect to sea-level.

The Coast and Geodetic Survey determined the value of gravity on three of the uninhabited islands of the western part of the Hawaiian Islands, at Honolulu, and on certain islands of the West Indies. The isostatic reduction

of the island-stations indicates that the isostatic anomalies average about +0.060 for the three stations on the uninhabited islands. These anomalies are of the same order of magnitude and of the same sign as are usually found for gravity-stations on other oceanic islands. It is believed that the islands referred to above are of volcanic origin. Perhaps they are underlaid with very heavy material close to the surface. This condition or distribution of density would have a tendency to cause excessive gravity.

An engineer of the Survey is making gravity-observations in the West Indies designed to supplement the gravity-at-sea work done on the Submarine S-21 of the United States Navy by Dr. Vening Meinesz during the fall of 1928. Dr. Meinesz, Member of the Dutch Geodetic Commission, was invited to this country by the officials of the Carnegie Institution of Washington, and the United States Navy placed at his disposal a submarine and two Eagle boats for the purpose of enabling him to determine values of gravity in the Caribbean Sea, the Gulf of Mexico, and in the Atlantic Ocean off the southeastern coast of the United States. Dr. Meinesz and his associates, Dr. Fred E. Wright of the Geophysical Laboratory of the Carnegie Institution of Washington, and Elmer B. Collins, Senior Scientist of the Hydrographic Office of the United States Navy, determined the value of gravity at 49 stations. The isostatic reduction for these stations was made at the office of the Coast and Geodetic Survey. (The results are reported upon in a paper entitled "A gravity-measuring cruise of the U.S. Submarine S-21" by F. A. Vening Meinesz and F. E. Wright, with an appendix on computational procedure by Miss Eleanor A. Lamson. This is in press as App. 1, v. 18 Pub. U.S. Naval Obs., Ser. 2, Govt. Print. Office, 1930.)

A matter of importance in the geodetic field was the determination by Dr. Meinesz with his pendulum-apparatus of the difference in gravity between De Bilt, Holland, and the base gravity-station in Washington, D. C. Dr. Meinesz had previously connected De Bilt with the fundamental absolute gravity-station at Potsdam, Germany.

Another connection between the gravity-stations in Potsdam and Washington was made by A. H. Miller, gravity specialist of the Dominion Observatory, Ottawa, Canada. Mr. Miller, during the latter part of 1928, had connected the Ottawa station with the one at Potsdam and, at the request of the Director of the Coast and Geodetic Survey, Dr. R. Meldrum Stewart, Director of the Dominion Observatory, permitted Mr. Miller to determine the difference in gravity between Ottawa and Washington. When the results by Mr. Miller and Dr. Meinesz are known, we shall have a stronger value for the base gravity-station in Washington, although the probable error of the present one seems to be small.

The Bilby portable steel towers were used on most of the triangulation executed during the past year and were found to be most effective and economical. These towers were first used on triangulation in Minnesota during the season of 1927. On the work in 1928, in Ohio, it was found that the trees were so high that the tower had to be increased to a height of 103 feet, by the addition of two lower 13-foot sections. On the work in Iowa, Missouri, and Arkansas, it was found necessary to add one lower section, thus increasing the height to 90 feet, in order to overtop the timber in that country and to make the lines of sight intervisible.

Very satisfactory results are being obtained for the accuracy of the observations. The average closing-error for the triangulation during the year was begween 0".8 and 0".9. It is interesting to note that the engineers of the Survey are directed to strive for an average closing-error of the triangles of about 1". This means that the sum of the angles of a triangle as observed

differs not more than about 1" from 180° plus the spherical excess. Of course, an accuracy greater than this is desired if it can be secured without increasing the unit-cost of the work.

It is believed that first-order triangulation, with a closing error of about 1", with frequent bases and Laplace stations to control the directions, is of sufficient strength to meet all practical and scientific requirements, except in very special cases, such as were encountered in the determination of the distance between San Antonio Peak and Mt. Wilson in southern California for the velocity-of-light determinations by Dr. A. A. Michelson. In that work many precautions were taken to have the accuracy of the triangulation greater than that usually obtained for the triangulation-net of the United States.

Astronomic determinations of latitude and longitude were made at a number of triangulation-stations in Iowa, Minnesota, and Wisconsin. The longitude-work was designed to furnish data for deriving Laplace azimuths to be used in the adjustment of the triangulation. The latitude-observations will furnish deflections of the vertical in the meridian for use in studying the distribution of densities in the outer portion of the crust, in isostatic investigations, and in future determinations of the figure of the Earth. The longitude-determinations will also be equally valuable for these investigations.

For some years the Coast and Geodetic Survey has been using the time-signals sent from the Naval Observatory in its longitude work. These signals are recorded on a chronograph at the field stations in order that an accurate comparison of the observatory and the local times may be made. The accuracy of the time-service of the Naval Observatory is exceedingly high; in fact, it meets all requirements for the very exact longitude work carried on by the Coast and Geodetic Survey. During the year, the Naval Observatory has initiated the sending of time-signals at 3 a.m. in addition to the signals previously sent at noon and at 10 p.m. The new time-signals make it possible for the field observer to make two determinations of longitude on a single night, thus shortening his stay at a station; or, should meteorological conditions be unfavorable for the local-time observations during the early part of the night, a determination of longitude can be made after midnight if observations are then possible on the stars.

The building in which the Coast and Geodetic Survey is now housed will soon be torn down in order to make way for a new office building for the House of Representatives of Congress. This will destroy the gravity base-station which has been in the basement of the Coast and Geodetic Survey for the last 30 years or more. At the time this paper is being written, observers of the Survey are transferring the base-station to the building at 119 D Street, Northeast, Washington, D. C., to be occupied by the Survey. Connections will also be made between the base-station at the present office of the Survey and the gravity-stations at the Smithsonian Institution and the Bureau of Standards.

U. S. Coast and Geodetic Survey,
Washington, D. C.

SECTION OF SEISMOLOGY

(Washington, D. C., April 26, 1929)

EARTHQUAKE SURFACE-WAVES

J. B. Macelwane

Before the time of Lord Rayleigh the mathematical theory of elasticity did not provide any theoretical basis for the existence of surface-waves in an elastic solid. Consequently, attempts were made to explain the so-called principal or main phase in the earthquake records by reflection, scattering and interference of body-waves. Such hypotheses failed to account for the difference of velocity between these waves and the body-waves and for the fact that the travel time of the main phase was observed to be approximately proportional to the distance of the observing station from the epicenter of the earthquake. In 1887 Rayleigh published in the Proceedings of the Mathematical Society of London a communication on "Waves propagated along the surface of an elastic solid" in which he presented a theory which was further developed by Lamp in 1904. These waves have since been designated Rayleigh waves. According to the theory the energy of the wave dies out rapidly with distance from the surface. The particles vibrate in elliptical paths whose major axes are perpendicular to the surface and whose minor axes lie parallel to the direction of propagation of the wave. The ratio of the vertical to the horizontal amplitude is nearly one and one-half. The velocity of Rayleigh waves is less than that of true shear waves in the same medium. The ratio of velocities is approximately ninety-two hundredths.

In the years that followed there were observed on the seismograms many instances of waves with no vertical component, but with a large horizontal component perpendicular to the direction of propagation. Such waves could not be accounted for by the Rayleigh theory.

In 1907 Wiechert published his hypothesis of transverse vibrations of the crust of the Earth on a semi-fluid layer of magma. He supposed the entire crust to be set in vibration in such wise that the upper and lower surfaces were traveling in opposite directions and that there existed a nodal plane in the middle, which remained at rest. However, his colleague, Zoeppritz, before his death in 1908, prepared a manuscript which was not published until 1919, in which he showed that Wiechert's transverse vibrations could not exist. They would be damped out at once.

In 1911 Love published an essay in which he proved that true transverse vibrations could exist as surface-waves provided the surface-layer were separated by a discontinuity from a medium of different properties immediately below it. Such waves would have no vertical component. The particles would vibrate in paths parallel to the surface and perpendicular to the direction of propagation. The velocity of these Love waves would lie between the velocity of true shear-waves in the upper layer and that in the lower layer, and would be a function of the depth of the surface-layer. This theory was hailed at once as the complement of the Rayleigh theory, and as offering together with it a complete explanation of the surface-waves observed in the records of earthquakes.

On the basis of the Love theory Gutenberg, Jeffreys, and others proceeded to calculate the distance of the surface-layer by means of the observed velocities of the transverse waves. To many seismologists, however, such calculations seemed an unwarranted assumption of the completeness of the theory.

The correspondence between the characteristics of the observed surface-waves and those demanded by the two theories is far from satisfactory. In the case of vibrations in the plane of propagation the required ratio of vertical to horizontal amplitude is seldom observed. Usually the horizontal amplitude is larger than the vertical. Besides, there is often a component of vibration perpendicular to the plane of propagation. One might suppose that we are dealing in such cases with a superposition of the Love type of wave on the Rayleigh type, but it is difficult to reconcile the amplitude ratios or the velocities with such an hypothesis. In the case of waves with no vertical component there is frequently present a large component of motion in the direction of propagation which is not allowed by the Love theory.

In 1918 a new theory of elastic surface-waves was published by Uller which deserves far more attention than it has received from seismologists. Uller attacked the problem from a more general point of view than either Love or Rayleigh. The results of his investigation were briefly that four types of surface-waves may exist at the boundary of a homogeneous elastic solid. The first is a pure shear-wave in which the particles vibrate in planes parallel to the surface so that there is no vertical component. No substratum or discontinuity is required. Hence we see how premature were the conclusions based on the Love theory. The second is a wave-type of which the Rayleigh wave is a particular case. It involves a strong vertical component. The third and fourth types of waves are of complicated form involving the coupling of both dilatation and shear. In their simplest form they make the impression of dilatation-waves vibrating in the direction of propagation, having superposed upon them moderate shear-waves with vibrations perpendicular to the surface. This more general theory is a long step forward but does not yet take care of all of the characteristics of the waves which are observed in the surface-phase of our seismograms. This statement remains true even when we take account of the possibilities which were proven by Uller that true dilatational surface-waves may appear as transients, and that dilatation-waves may exist in which the dilatations are perpendicular to the direction of propagation and shear-waves may exist in which the shears are parallel to the direction of propagation. The records of earthquakes which have occurred directly south of Saint Louis give us an opportunity of studying some of these interesting characteristics.

A further difficulty in our theory has been the question of absorption. Uller has proved that even in conservative media there may be an inherent decrease of amplitude due to the wave-form and parallel to the surface which corresponds to the previously known decrease of amplitude with distance and which leads ultimately to the extinction of the wave. We are thus forced to speak of a coefficient of extinction instead of a coefficient of absorption if such terms have any consistent physical meaning at all.

In such a visco-elastic medium as the Earth, according to all the theories, there ought to be dispersion and usually normal dispersion. But the results obtained by observation are not uniformly in favor of such a phenomenon. Different velocities have been observed for surface-waves of widely different periods but this is not equally true for waves of the same order of frequency. A further difficulty arises from gradual change of frequency and of wave-length without change of velocity. Such a transformation is not provided for in any of the theories. Uller has shown that the velocity and wave-length depend not only on the time characteristics of the source but also on the static field through which the wave passes. So that there is a change of wave-form and of velocity when a wave swings from a horizontal to a vertical path in the gravitational field of the Earth. And Sezawa's work seems to show that any motion

in an elastic viscous medium, such as the Earth, will gradually develop into waves which grow in length and decrease in amplitude. In fact, it was stated by Love that the oscillatory character of the surface-waves of earthquakes must be almost entirely due to the dispersive character of the medium. But here again the general theory of Uller gives us an advance over the previous work; for he thinks that the wave-length would increase for purely kinematic reasons in any medium.

However, satisfactory as these results are, they do not represent the whole truth. It is certain that the length of waves that carry the maximum amplitudes gradually and somewhat irregularly increases with distance from the epicenter as shown in the case of the California earthquake of 1922. But it is equally true that other waves of extremely long periods decrease even more rapidly in period and wave-length as was shown by Father Repetti in the case of the South Pacific earthquake of 1924. Nor is this phenomenon restricted to surface-waves. It was shown to occur in dilatational waves in the case of the California earthquake of 1922.

Thus we see that while there has been gratifying progress in the development of the theory of surface-waves in the last 40 years since the publication of Rayleigh's paper, still we are far from being able to explain all that we observe in the principal phase of our seismograms. Not only will it be necessary for the mathematicians further to develop the theory but further observations and exact measurements are required on the part of seismologists and particularly in the case of earthquakes whose direction of propagation coincides with the meridian or with the prime vertical at the observing station. For really conclusive results a vertical component seismograph and astronomically correct time are required.

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THE FORCES AND MOVEMENTS AT THE EARTHQUAKE-FOCUS

Harry Fielding Reid

It has long been known that tectonic earthquakes are due to fractures of the rock, usually connected with the formation of a new fault,or with renewed movement on an old one. The study of the California earthquake of 1906 developed the elastic-rebound theory, which explains in detail what occurs just before, and at the time of, the fracture. The theory was suggested by the displacements of various stations, as determined by Hayford's surveys; but it is not dependent on these displacements; it is founded on the fundamental characteristics of elastic solids. If we imagine stresses set up in the rock by movements in the Earth's crust, elastic strains will develop. If these strains pass the elastic limit, slow plastic distortions will take place. If the stresses grow sufficiently strong the rock will fracture and the two sides will spring back towards positions of no elastic strain. The elastic limit is a function of the time; the longer the stresses act the lower is the elastic limit; so that stresses not nearly strong enough to cause fracture may in time cause large plastic distortion, as evidenced by the slow bending of marble grave-slabs, and by the strong folding of strata.

The fracture does not occur simultaneously at all parts of the fault. The strength of the rock must be slightly different in different parts, and the growing stresses must also vary along the fault; they will first reach the

strength of the rock in a very limited area, where the fracture will begin. The elastic stresses, being no longer supported in this area, will be thrown upon the adjoining rock, which in turn will break; and thus the fracture will be extended until it reaches a region where the stresses are not strong enough to carry it forward. The speed of propagation of the fracture cannot be greater than that of compressional elastic waves in the rock, for they both depend on the density and elastic constants of the rock. Indeed it is sure to be less, for a part of the energy will be expended in fracturing the rock.

We see that it is most improbable that fractures should start simultaneously in different parts of the same fault or in different faults. Although movements sometimes occur at about the same time on two faults not far apart, the observations are not accurate enough to show that one of them may not have been started by vibrations from the other. Davison has described what he calls twin earthquakes, in which two fractures occur at so nearly the same time that neither could have been induced by the other; and he thinks this type is characteristic of many large earthquakes. Although not impossible, the frequent occurrence of twin earthquakes seems to me too improbable to be lightly accepted.

In the immediate neighborhood of the fault the rock may be considerably shattered and small blocks may be displaced; but the explanation of earthquakes as due to the sudden readjustment of large blocks of the Earth's crust does not seem accordant with the elastic-rebound theory; and in spite of a very thorough search for just such movements, no displacements except those in the San Francisco fault were found in California in 1906.

It was formerly thought that the throws of large faults were the result of a slow and continuous movement. But many earthquakes have been traced to the sudden spasmodic movement of an old fault, and now it seems fairly evident that the throw grows by a series of comparatively short steps. Many mountain ranges are limited by boundary faults and we may picture to ourselves the elevation of the mountain and the increased throw of the fault as follows: The vertical forces which raise the mountain may be continuous; they raise the mass continuously and develop increasing shearing stresses near its boundary. These stresses finally cause a fracture with an upward elastic rebound on the mountain side of the fault and a downward rebound on the opposite side. As time goes on mineral deposits heal the breach and the rock is made a continuous solid though not as strong as before. The vertical forces continue to raise the mountain; new shearing stresses are developed near its boundary until another fracture is made with rebounds of the two sides. And this process may be repeated many times until the mountain reaches a considerable height and the fault has a large throw. To the geologist the mountain has all the appearance of having been raised as a rigid block, but the actual process has been quite different. If the mountain has more than one boundary fault, these faults would have similar histories, but we should expect the fracture to occur at different times on the different faults.

Submarine earthquakes present phenomena of their own. If the movement on the fault is parallel with the bottom no material disturbance of the sea follows; but if there is a strong vertical movement, one side of the fault flinging up and the other down, great waves are developed which often cause great havoc when they reach the coast. At the bottom there will be a great rush of water over the cliff suddenly brought into existence, carrying the bottom-mud with it, and breaking and tangling any cables near the fault; though the cables may be broken by the fault movement itself. (It is also to be noted that submarine landslides will have a similar effect on cables. John

Milne looked upon them as a cause of earthquakes, but I do not think they satisfy the requirements for the production of strong vibrations in the solid crust.) The water on the upthrow side will be raised and that on the downthrow side will be depressed, and great waves extending through the whole depth of the water will be started, the elevation leading on one side and the depression on the other. There is an impression that when the waves reach the coast, the water first withdraws and then rushes in; but this is by no means generally true. The most extensive study we have of such waves are those of von Hochstetter and von Tschudi in connection with the Arica earthquake of August 13, 1868, and of Geinitz in connection with the Iquique earthquake of May 9, 1877. They collected reports of the incoming waves at many of the South American ports, at many islands in the Pacific, and at stations on the eastern coasts of Australia and Japan. An examination of these reports has led me to the following conclusions: At the time of the Arica earthquake the water first withdrew all along the South American coast and then advanced; but at points in the Pacific Ocean and the west, the waves began with an advance. This suggests a fault parallel with the South American coast, with the downthrow on the eastern side. At the time of the Iquique earthquake, the water first advanced at points of the South American coast north of Antofagasta, Chile, and first withdrew at points south of Antofagasta. At points in the Pacific Ocean to the west, the information is not entirely satisfactory but apparently the wave began with a recession. This suggests a fault running approximately northwest with the downthrow on the southwestern side. Although these observations are not sufficient to prove the correctness of our explanation, they are very suggestive, and it is hoped that a further study of the question, with reliance based more on tide-gauge records, will yield more satisfactory results. In following the wave from its origin to the coast-station, the varying depth of the sea must be considered, as this may bend the wave in such a way that the first movement of the water at the station may be the opposite to what one would expect from a casual glance at the map.

The weight of the overlying water must have some influence on the movement of the two sides of the fault. The force necessary to raise the water on the upthrow side will necessarily slow down this movement so that elevation at the surface of the sea will spread over a considerable area. The weight of the water may or may not increase the speed of the downthrow-movement, and we should expect the depression at the sea's surface to be less extensive than on the raised side. It is possible that a first elevation of the water at a coast-station may be too gentle to be noticed, especially when it occurs at night, and the more accented recession be interpreted as the beginning of the movement. This may be the explanation of the general belief that the waves begin with a recession.

So far we have dealt with shearing stresses, but there are cases where fractures have been caused by tension. They are unimportant as causes of earthquakes for they do not extend deep into the Earth. They may be caused by compression of horizontal strata, which then act as a strut and, yielding by bending, develop tension on the convex side. They may also be caused by lacoliths pressing up the overlying strata. I think the greater number of so-called volcanic earthquakes are due to tension-cracks. The pressure caused by upwelling lava must produce cracks, which the lava fills, and which appear later as radial dykes about partially disintegrated volcanoes. The strong but very limited earthquakes that occur on Mt. Etna often follow straight lines along which lava shortly afterwards pours out. It is difficult to imagine that the lava could be forced up in the vent of a volcano without causing cracks in the flanks. The small amount of energy in volcanic earthquakes follows directly from the shortness of the crack and the small thickness of

the rock involved. There have been some strong earthquakes connected with volcanic outbursts, but they are so rare that they must be regarded as incidental and not as a necessary part of volcanic action. Strong explosions are not rare at the time of volcanic outbursts but the strongest, such as those of Krakatoa in 1883 and Mt. Pelée in 1902, caused only insignificant earthquakes; and we must conclude that the usual volcanic explosions are less effective still.

Downfall-earthquakes is another possible but unimportant group. There is but one case that deserves attention, namely, the Pamir earthquake of February 18, 1911. The whole top of a mountain slid off into the valley 700 meters below, accompanied by a severe earthquake which was recorded at distant seismological stations. The question arose: Did the landslide cause the earthquake or did the earthquake cause the landslide? Galitzin, Klotz, and Jeffreys calculated the energy in the vibrations and the loss of potential energy in the downfallen mass, and found that they were of the same order; they concluded that the landslide caused the earthquake. Oldham found that the region of greatest intensity did not coincide with the position of the landslide and concluded that the earthquake caused the landslide.

Landslides do not generally produce noticeable earthquakes; they do not fall and strike the valley as a solid mass, but break up into fragments and deliver lighter blows distributed over many minutes of time, which are not adapted to set up violent vibrations. These considerations and Oldham's observation lead me to agree with his conclusion.

When the fracture of the rock takes place the shearing stress at the surface of the fracture is suddenly annulled, and the rapid fling of the rock begins at this surface; this is the starting point of the vibrations which are propagated to a distance. It is evident that in front of the moving rock a condensational wave will be developed, and in its rear a dilatational wave. The San Andreas fault which was responsible for the earthquake in California in 1906 trends roughly northwest; the northeastern side flung to the southeast and the southwestern to the northwest; so that in the northern and southern quadrant a condensational wave must have led, and in the eastern and western quadrants a dilatational. In the case of a dip-slip fault a condensational wave would start downward on the downthrow-side and a dilatational wave on the upthrow-side; the opposite type would advance to the surface of the Earth and there suffer reflection with change of phase. Evidently there is an opportunity here for some complications, but in general the Earth's surface will be divided into two parts, in one of which the disturbance will begin with a condensational wave and in the other with a dilatational. The dividing line could be worked out in some such way as that recently developed by Byerly. Records of an earthquake in many seismological stations would thus throw light on the character of the responsible faults. (Some years ago Nakone showed mathematically that a sudden impulse in an infinite medium would start waves with condensation leading in one-half the medium and dilatation in the other half.)

The stresses which finally cause the fracture of the rock are clearly developed by differential movements of adjoining areas; but the cause of these movements is, with one exception, quite unknown. The exception is a mountain mass undergoing erosion. There the readjustment of isostatic equilibrium causes a continual elevation of the mass, developing stresses about the boundary which may result in faulting.

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THE VELOCITY OF SURFACE-WAVES

Frank Neumann

(Abstract)

The paper (published in full in Bull. Seis. Soc. Amer., v. 19, 63-76, 1929) deals with methods of interpreting instrumental records of seismic surface-waves to determine the velocities of the several wave-groups. Current methods of identifying surface-waves depend upon characteristics of period and amplitude only and generally no attempt is made to distinguish between the two major types of surface-wave motion, namely: (1) Transverse motion of Love wave-type and (2) motion of Rayleigh wave-type in which the motion of the horizontal component is in the line of propagation. The key to the methods of identifying wave-types lies in determining from the grams the motion of the earth-particle by practical methods. The three methods discussed depend upon (1) the ratio of instantaneous amplitudes on the two horizontal components, (2) phase-relationships between instantaneous displacements on the two components, and (3) transformation of the observed motion to a new system of coordinates which are rectilinear with respect to the direction of propagation.

It has been found that dominant wave-groups are usually subject to successful interpretation upon the application of one or more of these methods. Secondary groups are more likely to be complex as distinguished from the prevailing harmonic character of most dominant wave-groups. Progress in the way of special investigations has been limited to a study of Honolulu seismograms using the second or amplitude-ratio method.

It has been found that (1) dominant wave-groups may be of either Love wave or Rayleigh type, (2) that the velocity of either type depends upon the path, and (3) that the several observed velocities correspond closely to velocities of interior S-waves which penetrate the two outer layers of the Earth's crust and the normal S-wave traveling near the surface of the rock-mantle. This indicates that surface-waves may be propagated through possibly three different media, and it appears that each one may be capable of propagating both Love and Rayleigh wave-types.

It appears that application of the methods described may result in early and successful disintegration of the surface-wave phase into its elementary types. This would settle vital questions regarding surface-wave propagation and variations in crustal structure, and in all probability would yield significant data on the nature of seismic foci.

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THE SEISMICITY OF THE ARCTIC

Ernest A. Hodgson

(Abstract)

The paper (to be published in full in J. R. Astr. Soc. Can.) presents an analysis of the data compiled at the Dominion Observatory, Ottawa, Canada, in connection with the series of publications on the "Location of epicenters

for the years 1911-1927, inclusive, together with that given in the series of "International summaries" published by Prof. H. H. Turner of Oxford for the years 1913 to July 1925. The analysis was undertaken for the purpose of determining the recorded seismicity of the arctic. The above sources of information are further supplemented by the earlier instrumental records of John Milne (1899-1906) and by several catalogs of earthquakes which yield information particularly with regard to Iceland, Jan Mayen, and Spitsbergen.

The conclusions with regard to the earlier data are as follows: (1) Of the entire arctic region, only that part lying between Iceland and Spitsbergen registered as seismic on the Milne seismographs for the period 1899-1906; (2) historic records of earthquakes in Iceland (1013-1899) and the instrumental records obtained at Reykjavik (1910) show that the island has distinct seismic areas which suffer repeated shocks of a more or less limited nature, which are due to volcanic action; (3) the larger earthquakes--the tectonic, world-shaking tremors--do not seem to be directly associated with volcanic action; (4) tecton-

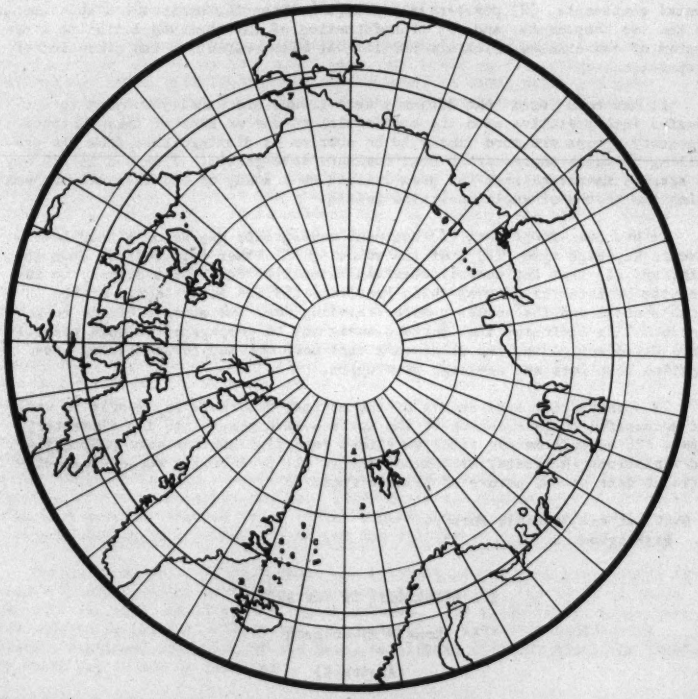

Fig. 1--Epicenters of arctic earthquakes, 1911-1927

ic earthquake which occurred 250 km east of Spitsbergen, of intensity 7 to 8 (Rossi-Forel) at Spitsbergen, is just barely registered on a Bosch photographic seismograph at Ottawa--a distance of approximately 5,500 km.

The deductions from the more recent data of the Oxford and Ottawa publications are tabulated. A discussion of the tabulation shows that: (1) Nearly all the smaller records are from epicenters near Iceland or Jan Mayen--probably these are due to volcanic rather than tectonic earthquakes; (2) of the well-determined epicenters, presumably of tectonic earthquakes, all but three lie in the Jan Mayen-Iceland region, being concentrated near Jan Mayen; (3) one of the two other seismic areas designated within the Arctic Circle by almost 16 years of uninterrupted recording is at approximately 77° north latitude and 127° east longitude, off the mouth of the Lena River, Siberia, at which point two earthquakes occurred within about nine hours of each other, at practically the same epicenter, on May 30, 1923--the independent locations by Ottawa and by Oxford agree within close limits; (4) the remaining seismic area of the arctic is represented by the earthquake of November 16, 1920, at approximately 72° north latitude and 127° west longitude, north and east of the mouth of the Mackenzie River, Canada--the independent locations by Ottawa and by Oxford agree almost exactly and there can be no doubt whatever regarding the accuracy of this location.

The paper also discusses a series of records registered at Ottawa, which seem to indicate a fairly active region for small tremors north of Ottawa. A list of more than a hundred such records from July 1924 to May 1929 is given. The observations made are that such records, in general, present the following characteristics: (1) The duration of the tremors is seldom more than five or ten minutes; (2) the north-south component is usually the more pronounced; (3) a value for distance is almost never possible, but they come apparently from epicenters at a distance of a thousand kilometers, less or more; (4) no press-reports of tremors within this distance agree with the times of the earthquakes (there is one possible exception--that of No. 3316, December 2, 1928, reported in the press as having been felt in northern Ontario; the date is uncertain in the press report).

In conclusion, a statement is given as to the seismic stations lately established or which are to be established by Denmark, in or near the Arctic region. A short bibliography is appended in the complete paper.

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EARTH-VIBRATIONS FROM DYNAMITE BLASTS

L. Don Leet

As is pretty generally known by now, there has developed in recent years a commercial activity which involves the setting up of elastic vibrations in the Earth by means of dynamite blasts. This work, generally known as "Seismic prospecting," has for its aim the study of subsurface geologic structure by means of these vibrations, following the theory by which seismology offers means to a study of the Earth's deeper interior. The greater part of such commercial investigations, however, has had for its more limited objective the location of salt-domes as favorable structures for the accumulation of oil.

This activity, though now stressed because of its commercial possibilities,

is an outgrowth of purely scientific interest in the behavior of elastic vibrations in the Earth's outer crust and offers a means of indirectly furthering the aims of that interest. Perhaps the critical impulse most directly leading to its development was the effort made during the war to use seismic means of locating enemy guns. The first parties successfully applying the method commercially came from Germany.

The major contribution made by commercial activities of this nature so far has been the development of specially adapted instruments and an efficient technique for making observations.

It is the purpose of this discussion to consider on the one hand advances which seismic prospecting has made in field methods of studying the vibrations from dynamite blasts, and on the other hand possibilities, inherent in the work, of important contributions to the theory of such studies. Incidental to the theoretical considerations is a widening of the field of applicability of seismic prospecting which would naturally result from a proper solution of fundamental problems of theory.

The first records of controlled experiments involving vibrations originating from artificial explosions are those of Robert Mallet, the British engineer, in 1853. He was one of the first to recognize the importance of the dependence of seismic velocities on rock-constants and to suggest that the knowledge of these velocities and rock-constants would enable us to predict the nature of the underlying rock-formations. The development of this idea has made seismic prospecting possible.

His first experiments were made in two widely differing materials--the wet sand of Killiney Bay, on the coast of County Dublin, and the granite of the neighboring Dalkey Island. In Killiney Bay, charges of 25 poundsof gunpowder were exploded in the sand, and the passage of the waves at a distance of half a mile was detected by the tremors on the surface of a mercury bath. In Dalkey Island, the ranges were of less than half this length (1,021 to 1,155 feet). The mean of eight measurements in sand, six in fissured granite, and three in more solid granite, gave the following velocities, after corrections were applied: Sand, 825 feet per second; discontinuous granite, 1,206 feet per second; more solid granite, 1,665 feet per second.

Later, at Holyhead from 1856 to 1861, he came to the conclusion that the transit velocity increased with an increase in the intensity of the initial shock.

General Henry L. Abbot in 1878 essentially confirmed Mallet's statements of principles, though he obtained velocities of much greater magnitude.

Modern experiments have, of course, demonstrated that many of the principles deduced by these investigators, particularly that of the relation between velocity and energy of detonated charge, were incorrect due to observational errors. There is no need to discuss these here.

Recent experimenters with the same objective, however, still admit uncertainties of considerable degree in their results, in spite of modern equipment employed. The work in May 1924 of Maurain, Eblé, and H. Labrouste in studying four controlled explosions at La Courtine, France, is a case in point. Charges I and II of five, and III and IV of ten tons of melinite were used, and the vibrations recorded at La Courtine, Pelletin, and Ussel, at distances varying from 5,590 meters to 24,910 meters. Blasts I and III were

in granite, and II and IV a short distance away in gneiss. The wide variation of as high as 20 per cent in P-velocities led to a division of the results into two groups on the astonishing assumption that the vibrations from I and III, starting in granite, exhibited a strong attachment for that medium and so transmitted themselves that their velocity was governed largely by the granite, and they deviated from the plane including shot-point and recording station. Even after this drastic treatment, the results displayed discrepancies as great as 10 per cent in P-velocities, a rather large factor if any extrapolation from the results is to be made. Mean velocities of 5,520 meters per second for P and 2,800 meters per second for L were the values finally adopted.

By way of comparison with magnitudes and constants involved in seismic prospecting, certain details are of interest. Horizontal components had periods of 0.5 and 0.6 and a vertical 0.17 of a second at La Courtine, and of 4 seconds at the other two stations. Damping was made "feeble" in order "to render the instruments very sensible." This naturally rendered interpretations of other than first waves unreliable. No reproductions of records accompanied the report. The period of ground movements was determined as from 0.1 to 0.2 second for P and L, the two types studied. Static magnifications of the instruments at La Courtine were 1,000, 600, and 450, at Ussel 196 and 194, at Felletin 238, increased by the introduction of optical recording for the last two blasts to 7,200 and 5,600.

Periods of 0.1 and 0.2 for the velocities adopted as mean values indicate wave-lengths of the order of 550 meters.

Recording paper-speeds were 1 cm per second with time-marks imposed at half-second intervals. An electric circuit joining the three stations imposed absolute time on the records simultaneously. The instant of the blast was determined differently at each shot, but the time finally accepted was that registered from a precision chronometer (apparently by eye-comparison, though this is merely inferred) at the firing point, which was synchronized with the timing circuit joining the recording stations. In other words, there was not any direct registration on the records of the instant of the blast.

Valuable information has on occasion been obtained from accidental as well as controlled explosions. A great explosion which occurred at the works of the Badische Anilin and Sodafabrik at Oppau, in the Bavarian Palatinate, on September 21, 1921, was the subject of an investigation by Dorothy Wrinch and Harold Jeffreys. From the records of six seismograph stations, velocities were computed and certain deductions made. Here, of course, the exact instant of the blast was not determined explicitly. The report of this study does not include reproductions of records or information regarding the recording instruments, so details of record-interpretation must be taken on faith. Seismologists will recognize the danger of this.

The stations whose records of the Oppau explosion were studied were at distances from 110 km to 365 km. The velocity of P-waves was determined as 5.4 km per second, and of S, 3.15 km per second. At the two nearer stations, the period of L ranged from 1.7 to 3 seconds, implying wave-lengths of about 6 km and vertical extent of about 15 km.

The first use of controlled, artificially established vibrations for studying geologic structures was made by Fessenden in 1913, when he conducted a series of experiments at Framingham, Massachusetts, which led to the patenting of his methods. It was, however, in Germany that the necessary thoroughness was available to carry the method through to successful commercial

application. In the early summer of 1923, a German party started work for the Royal Dutch Shell Company in Mexico. Late in 1924, a troop of the same German company, working for the Gulf Production Company in Texas, brought seismic prospecting into real repute by the discovery of several salt-domes. Since that time, other companies have developed similar methods, and rapid strides have been made in the improvement of technique, with increasingly profitable results.

Every record taken in seismic prospecting-work is essentially an experimental determination of vibration-velocities. The necessity for rapid results has led to the development of an efficient technique which would be as well adapted to similar investigations in the interests of purely scientific research of a similar nature. Perhaps the best method of presenting this technique is by means of a brief description of field methods employed at present.

Many details of operations depend, naturally, on the nature of the recording instruments. Tromometers of practically every kind are in use, from different types of mechanical pendulums with optical registration, to galvanometrically recording instruments sometimes called electric self-induction geophones. Most companies record only the vertical component, some record one horizontal and the vertical.

The methods here to be described are those adapted to the use of galvanometrically recording geophones, as already described by D. C. Barton in his paper "The seismic method of mapping geologic structure" (Amer. Inst. Min. Metall. Eng., Geophysical Prospecting, 572-624, 1929). These geophones have the widest range of usefulness of any instruments now employed and can be used equally well on dry land or under water.

Field methods

In the Gulf Coast, search is made for salt-domes as favorable structures for the accumulation of oil. These cylindrical plugs are intruded into sediments which transmit vibrations with comparatively low velocities, of the order of 1,800 to 3,000 meters per second, while the salt transmits them normally with velocities in the neighborhood of 5,000 to 5,500 meters per second. By plotting time-distance curves, it is accordingly a comparatively simple matter, theoretically, to identify this higher velocity and consequently a salt structure.

As a result, field observations are designed to yield two sets of values--the distances from shot to recorder, and the corresponding transit times for waves through the ground, for a net-work of locations covering the territory being studied.

Shooting--A single crew is assigned to handle the firing of dynamite charges, or "shooting," as it is called. In land work, a hole from 15 to 20 feet deep is customarily dug to receive the charge. A single shot ranges from 75 pounds to 450 pounds of dynamite, according to the distance to be covered. Sixty per cent quarry-gelatine is commonly employed. The firing is done by an electric circuit which includes a second charge of 16 to 33 pounds even as much as 75 pounds for longer distances, placed on the surface to set up the air-wave by which distances are measured. Around the main charge is wrapped a wire which leads to an electromagnet at a radio sending-key. When the signal to "fire" is received, the shooter sends a pair of dashes as warning, then depresses the key, which is held down by this electromagnet when it closes the circuit, sending out a continuous radio signal until the wire is

broken by the firing of the blast. In this way, the exact time of the explosion is marked on the observers' records where a line is made by the impulses received over the radio set.

Recording--Three recording outfits comprise a normal complement operating with one shooter. Economic factors and mobility of the troop are the only considerations which limit the number of observers. On land, the instruments of each observer are mounted on a half-ton truck. On water, the outfits are installed in 35-foot speed-boats capable of making as high as 35 miles per hour. These boats are specially constructed for the work.

The first of the instruments is the tromometer. The ones under discussion record the vertical component only. They are encased in metal cylinders about a foot in length and three inches in diameter. In land work, the tromometer is generally buried about three feet below the surface. On water, it is pushed into the bottom by means of a specially designed pronged pole. Laboratory experiments indicate that deviation of the tromometer's position from the vertical does not lessen its sensitivity more than ten per cent up to the point where it will not function at all, a condition easily detected by the observer.

The self-period of the instruments is of the order of a few hundredths of a second--0.03 to 0.06 and 0.07 are probably common values. The magnification varies, as will be pointed out. Oil-damping is used, with a variable ratio, probably seldom greater than 4 to 1.

The recording is galvanometric. That is, movements of the case relative to the mass, or the opposite, cause a small coil of wire to cut the magnetic lines of force of a permanent magnet. This generates an electromotive force which is transmitted by a cable from the tromometer to an electron-tube amplifier which steps it up. Possible magnifications range from a few hundred to a few million times. From there, it is conducted to a galvanometer whose mirror directs the beam from a light source onto the sensitized surface of a regular moving-picture film in the familiar way. This hook-up, obviously, makes it possible, by controlling the number of amplifier-stages and the type and sensitivity of galvanometer, to vary over a wide range the sensitivity of the recording circuit. In this way, records can be kept on scale when working near the shooter, by lessening the amplification, and the reverse. All of this is convenient in operation, but is done at the sacrifice of knowledge of the effective magnification of the system.

No attempt is made in field work to compute amplitudes of actual ground-movement. Accordingly, no attention is paid in the field to the maintenance of a given self-period or damping-ratio for the recording galvanometer, and their relation to the same constants of the tromometer. The writer has no information concerning the original adjustments made in the laboratory.

Another of the observer's instruments is a combined sending-receiving radio set. As mentioned above, the receiving circuit can be switched to a galvanometer which stands beside the geophone-galvanometer and records on the photographic strip variations in the plate-current occasioned by dots and dashes as well as the "time-break" in the long dash sent by the shooter just before firing. Observers are now required by law to be licensed operators (second-class commercial at least). The radio is used for all communication between units.

Since the photographic film is reeled by hand when the record is taken, some method is necessary to impose time-marks upon it which are independent

of the film-speed. This is done by means of calibrated forks, activated by a synchronous motor and vibrating at such a rate that each excursion past the equilibrium-point permits the passage of a beam of light to the film 50 or 100 times per second. The resulting impressions are narrow lines either at the top and bottom of the record, or extending across its width, as the particular adjustment may indicate. The latter is by far the more preferable. Readings can be interpolated to thousandths of a second on many records.

As each observer gets his geophone planted and instruments adjusted for taking a record, he calls the chief observer, or shooter, and reports himself ready. When all have reported, the signal is given to "shoot." As the observer hears this, he switches the radio onto its galvanometer, starts the timing-fork motor, and begins cranking. He can watch the geophone and radio-light spots through a ruby-glass opening in the camera, and stops cranking after the arrival of the air-wave has deflected the geophone-spot.

In the meantime, the observer's helper takes a compass-bearing on the shot-point, and with an anemometer observes the direction and velocity of the wind. A thermometer gives the air-temperature as the last of the data necessary to compute the exact velocity of the sound-wave through the air from shot-point to observer. Computation of this velocity, with the transit-time of the air-wave, gives an accurate determination of the distance from shot to recorder.

The film is removed from the camera and immediately developed, before a decision is made as to where the next record should be taken.

An observing unit can be set up ready for operation within 20 minutes, and can be packed ready to move in five minutes. With three records at least being made for each shot, this means that a large number of records can be taken in a single working day. Under pressure, as high as 20 stations, all in different places, one reported to have been occupied by a single receiving-unit in one day, on land. Even more are possible in work on open water.

Computing--The mechanical details of computation are handled by a single man for one party. When the records for a day are brought to him, his first duty is to "count" them. On each record, he establishes three primary points: The "time-break" marking the instant of the explosion; the "geophone-break" marking the arrival of the first wave through the ground; and the "air-wave."

From the time-break, the computer starts counting the timing-fork marks. At the geophone-break, he marks the time-interval from the time-break to that point, and the same for the air-wave break.

The next step is to bring into workable form the data on wind-velocity and wind-direction, air-temperature, and azimuth of recorders from shot-point for each shot. From these, the distances are calculated.

The plotting of time-distance curves is done to the nearest ten meters and one-hundredth second.

The importance of the computer's position depends in large part on the party chief. He can be turned into an automatic counting-machine, needing only a knowledge of elementary arithmetic. Yet he is often the only one who comes into direct constant contact, and acquires familiarity (or has an opportunity to) with all of the original records. This places him in the best

position to make valuable contributions by studying record-character beyond the first break, and other potential sources of information frequently ignored in the rush of field work.

Reconnaissance--The first step in entering new territory for a seismic survey is to determine the normal "ground" velocity of the region. This is customarily done by establishing a series of recording points in a single azimuth from the shot-point and running a straight line profile from that point. The points are generally fairly closely spaced, and the maximum extent of the profile is usually not over two or three miles.

A shot-point is then located at some convenient place, and records made by placing the observers at more or less uniform distances from this in a fan formation, that is, ten to fifteen degrees apart about the point. The radial distances from shot-point to observers range from three to as much as nine miles, depending on the nature of the terrain and the amplitude with which it transmits vibrations.

Theory

Discussion--The success of the seismic method in the discovery of salt-domes in the Gulf Coastal regions needs no comment. Many discussions of the peculiar structural set-up are available elsewhere and need not be repeated here. Accepting salt-dome reconnaissance as a closed field, for the moment, then, a discussion of theory should be directed toward the possibilities of the seismic method for the exploration of subsurface conditions which might lead to quantitative data on other structures than salt-domes.

There are several clearly definied issues which must be met before a wider applicability of seismic methods can be assured. Some will appear as axiomatic to one trained in the principles of pure seismology. Such an individual, however, would have to spend only one day in the field to realize the justice of their inclusion here in a discussion of present-day problems in seismic prospecting. This, of course, cannot be said to apply to all troops in the field. The writer, however, feels that it is a true generalization for the majority and is certainly true of methods now described in the literature.

(1) The path of the vibrations from shot to recorder--A preliminary discussion of this problem is undertaken by Maurice Ewing and the writer in a paper entitled "Seismic propagation-paths." A brief summary of the results will suffice for the present survey of the situation.

The literature and field methods to date have based theoretical formulas and interpretations on the assumption of superposed horizons which transmit vibrations with different velocities, but within each of which isotropy makes the vibration-paths straight lines. This isotropic condition must, of course, extend over ranges which are fairly large multiples of the wave-lengths involved.

A given ray starting from the shot-point is assumed to impinge upon a buried horizon at the critical angle, travel along its surface, and at the proper moment, assuming a Huygen's construction where each point of the advancing wave-front sets up new waves, to again enter the upper layer and travel to the recording instrument. Complete developments of these rectilinear propagation formulas may be found best presented in D. C. Barton's "Seismic method of mapping geologic structure."

These were, of course, recognized by those who developed them as at best

only approximations, though this is not always kept in mind by men in the field. There are serious doubts as to whether they are even rough approximations in many cases.

The elastic constants of rocks are known to change with depth, particularly in a sedimentary column. The prevailing sense of this change is such that elastic-wave moduli are increased, and elastic vibrations are transmitted with greater velocities. This increase is likely to be quite marked in a sedimentary column, which is at present the chief concern of seismic prospecting.

Assuming this increase to be linear with depth, Maurice Ewing has investigated the propagation-paths mathematically. He finds them to be the arcs of circles whose radii and line of centers for a given region are readily derived from two observations. It is then a simple matter to compute the depth to which a given ray penetrates.

Such observational data as have been available for a check give encouraging confirmation. It is to be hoped that further experimental data can be brought to a test of these formulas, which seem to be an important step in the right direction.

(2) Determinations of angles of emergence--This is a vital point in answering the fundamental question as to whether a refracted or reflected wave has been recorded.

(3) Equipment of each recording station with two horizontal-component instruments in addition to a vertical, preferably with all three recording on the same film--The increased cost, initially, and in operation, which these would represent is negligible in comparison to totals now being spent, when potential results are considered.

(4) Determination of instrumental constants necessary to calculate the energy of arriving waves, and actual ground-movements--The need for this is a direct result of the necessity for

(5) Theoretical investigation of relative energy-content of reflected and refracted rays in the key-horizons of a given region--A preliminary outline of this problem is given below.

(6) Introduce some fundamental principles of geology into the interpretation of results in the field--The importance of physical and mathematical considerations is heavily stressed in field work, particularly the former, as observational routine demands. On the other hand, potentially valuable results are vitiated by failure to consider known facts of geology. Too often, a knowledge of geology appears to be regarded by field workers as a cultural contribution of incidental interest, but no particularly immediate practical value.

In many cases, there seems to be too little community of understanding of respective problems between physicists making observations and geologists interested in the interpretation.

One last fact seems worthy of mention here as possible pertinent to either or both of two of these problems, the propagation-path, and the application of geology.

In certain areas, after an initial ground-velocity of, say, 1,800 meters

per second, there is encountered a "break" into one of around 2,100 meters per second. Sometimes another still higher is met beyond this. These have come to be known as the first and second "semi-consolidated" velocities, for lack of a genetic interpretation.

Two possibilities suggest themselves as working hypotheses that might be tested in seeking an explanation of these phenomena. If the true time-distance curve where no thick high-speed layer is present is a curve rather than a straight line, these apparent breaks in velocity might prove to be the meeting points of successive tangents to that curve. Or, there might be true changes in velocity over wide areas at the planes of discontinuity between Eocene and Cretaceous and between Cretaceous and Paleozoic sedimentation in the Gulf Coast, or other major discontinuities elsewhere.

Certainly, a solution of the question would seem to be worth attempting beyond a mere classification of the horizons as "semi-consolidated."

A problem in reflection and refraction of elastic waves

A matter of fundamental importance to the study of earth-vibrations from dynamite blasts is the behavior of incident vibrations at the plane-interface between two media of different elastic constants. C. G. Knott investigated this problem first 30 years ago, but the pertinence of his results to seismic prospecting seems to have been overlooked.

The greater part of the field work to date has involved a study of waves refracted through high-speed media. The Geophysical Research Corporation, however, has undertaken to make use of reflected waves as well, and others will no doubt follow their lead. An ability to do this, of course, would greatly increase the power of the method, so it seems worth while to consider in some detail the fundamental principles involved.

Quoting in part from "Reflexion and refraction of elastic waves, with seismological applications" (C. G. Knott, Phil. Mag., July 1899, p. 90 et seq):

Theoretical discussion of behavior of elastic waves at the plane-interface of solids--The equations of motion for plane-waves in an elastic solid are expressible in the form

$$(m + n)\nabla^2\varphi = \rho(d^2\varphi/dt^2),\ n\nabla^2\psi = \rho(d^2\psi/dt^2),\ n\nabla^2\chi = \rho(d^2\chi/dt^2) \qquad (1)$$

in which the plane XY is taken perpendicular to the wave front, and the displacements in directions X, Y, Z, are respectively

$$\xi = (\partial\varphi/\partial x) + (\partial\psi/\partial y),\ \eta = (\partial\varphi/\partial y) - (\partial\psi/\partial x),\ \zeta = \chi$$

in which, also, ρ is the density, n the rigidity, and $(m-n/3)$ the bulk-modulus or resistance to compression. [Note: Observe this value of $(m-n/3)$ for the bulk-modulus; elsewhere, it has been customary to use $(m-4n/3)$, where m is the longitudinal wave-modulus.]

The stress-components have the values

$$\left.\begin{aligned} P &= (m + n)\nabla^2\varphi - 2n(\partial\eta/\partial y); \quad S = n(\partial\zeta/\partial y); \\ Q &= (m + n)\nabla^2\varphi - 2n(\partial\xi/\partial x); \quad T = n(\partial\zeta/\partial x); \\ R &= (m + n)\nabla^2\varphi; \quad U = n[2(\partial^2\varphi/\partial x\partial y) + (\partial^2\psi/\partial y^2) - (\partial^2\psi/\partial x^2)] \end{aligned}\right\} \qquad (2)$$

where P, Q, R, S, T, U have the same meaning as in Thompson and Tait's "Natural philosophy."

The components of stress on the plane whose normal has direction cosines λ, μ, ν are

$$F = P\lambda + U\mu + T\nu, \quad G = U\lambda + Q\mu + S\nu, \quad H = T\lambda + S\mu + R\nu \qquad (3)$$

The waves of the φ-type are condensational-rarefactional waves traveling with a speed equal to $\sqrt{(m + n)/\rho}$. The waves of ψ-type are purely distortional waves traveling with speed $\sqrt{n/\rho}$, the vibrations being in the plane XY. The ζ-displacement belongs also to a purely distortional wave, the vibrations being at right angles to the plane XY. Let the plane-interface between two media, $m\rho$ and $n\rho$, be perpendicular to the X-axis, then any incident wave of the ζ-type will, in general, break up into two waves, a reflected wave of the same type. But any incident wave of the φ-type will, in general, break up into four parts, two distortional--reflected and refracted, and two condensational-rarefactional. Similarly, any incident wave of the ψ-type will also, in general, break up into four parts.

The necessity for this duplication of reflected and refracted waves may be easily shown by a simple consideration of the boundary conditions which must be satisfied at the interface. Even in the simple case of a solid bounded by an impassable barrier, we must assume the two reflected waves as derived from the one incident wave, or we encounter an absurdity.

This important principle does not seem to have been explicitly recognized in the literature of the subject, although Green's treatment of what Kelvin calls the "pressural" wave involves it. In 1887, Knott introduced the complete discussion of the problem in his lectures on elasticity to advanced students in the Imperial University of Tokyo, and made some definite calculations in regard to earthquakes in a paper which was read before the Seismological Society of Japan in February 1888 and published in its "Transactions."

In the September number of the Philosophical Magazine for 1888, Lord Kelvin gives the formulae for the case of the incident distortional waves, and discusses a similar problem in the February number for 1899, page 179. Below are the complete solutions for one case, the meanings of which are brought out by the definite numerical calculations which follow.

Condensational-rarefactional wave at the interface of two elastic solids--The solution is of the form

$$\left.\begin{aligned} \varphi &= Ae^{ib(\alpha x + y + wt)} + A_1e^{ib(-\alpha x + y + wt)} \\ \psi &= B_1e^{ib(\gamma x + y + wt)} \end{aligned}\right\} \text{in medium } m\rho$$

$$\left.\begin{aligned}\varphi' &= A'\varepsilon^{ib(\sigma' x + y + \omega t)}\\ \psi' &= B\varepsilon^{ib(\gamma' x + y + \omega t)}\end{aligned}\right\}\ \text{in medium } \underline{m}'\,\underline{n}'\,\rho'$$

where $\underline{\alpha}$, $\underline{\alpha}'$, γ, γ' are cotangents of the angles of incidence, refraction, and reflection. The equations of motion give the relations

$$\left.\begin{aligned}(m + n)(\sigma^2 + 1) &= \rho\omega^2 = n(\gamma^2 + 1)\\ (m' + n')(\sigma'^2 + 1) &= \rho'\omega^2 = n'(\gamma'^2 + 1)\end{aligned}\right\} \qquad (1)$$

The conditions to be satisfied at the surface ($x = 0$) are as follows: (1) Equality of normal displacements on each side of the interface, or $(\partial\varphi/\partial x) + (\partial\psi/\partial y) = (\partial\varphi'/\partial x) + (\partial\psi'/\partial y)$ when $x = 0$; (2) equality of tangential displacements on each side of the interface, or $(\partial\varphi/\partial y) - (\partial\psi/\partial x) = (\partial\varphi'/\partial y) - (\partial\psi'/\partial x)$ when $x = 0$; (3) equality of normal stresses on each side of the interface, or $(m + n)\nabla^2\varphi - 2n[(\partial^2\varphi/\partial y^2) - (\partial^2\psi/\partial x\partial y)] = (m + n')\nabla^2\varphi' - [2n'(\partial^2\varphi'/\partial y) - (\partial^2\psi'/\partial x\partial y)]$ when $x = 0$; (4) equality of tangential stresses on each side of the interface, or $n[2(\partial^2\varphi/\partial x\partial y) + (\partial^2\psi/\partial y^2) - (\partial^2\psi/\partial x^2)] = n'[2(\partial^2\varphi'/\partial x\partial y) + (\partial^2\psi'/\partial y^2) - (\partial^2\psi'/\partial x^2)]$ when $x = 0$.

These lead to the equations

$$\left.\begin{aligned}B_1 + \sigma Y &= B' + \sigma' A'\\ \gamma B_1 + X &= -\gamma' B' + A'\\ -2\gamma B_1 + (\gamma^2 - 1)X &= 2(n'/n)\gamma' B' + (n'/n)(\gamma'^2 - 1)A'\\ (\gamma^2 - 1)B_1 - 2\sigma Y &= (n'/n)(\gamma'^2 - 1)B' - 2(n'/n)\sigma' A'\end{aligned}\right\} \qquad (2)$$

where $X = A + A_1$ and $Y = A - A_1$.

By taking the difference of the products of the first and third, and of the second and fourth of these, and by suitable substitution according to (1), we get the energy-equation in the form

$$\sigma\rho A^2 - \sigma\rho A_1^2 = \gamma\rho B_1^2 + \sigma'\rho' A'^2 + \gamma'\rho' B'^2 \qquad (3)$$

The energy-equation shows how the original energy $\sigma\rho A^2$ of the incident wave is distributed among the four waves into which it breaks up at the interface. In the detailed numerical calculations of the ratios of A and B quantities in any particular case, the energy-equation supplies an important criterion of the accuracy of the work.

The equations (1) give the relations among the quantities $\underline{\alpha}$, $\underline{\alpha}'$, γ, γ' for any assumed values of the densities and elastic constants. Hence, for any chosen value of $\underline{\alpha}$, that is, for any chosen angle of incidence, the corresponding values of $\underline{\alpha}'$, γ, γ' are readily calculated; then the numerical values of the coefficients in equations (2) can be filled in. We are thus left with four simple numerical linear-equations from which any four of the five quantities can be determined in terms of the fifth.

(Note: It seems to make for greater speed and accuracy to fill in the numerical values in the equations as they stand, and then solve the equations for every individual case, rather than working the several algebraic expressions for each ratio, and then substituting. Except when $\underline{n}$ and $\underline{n}'$ are equal, or when

either vanishes, the expressions are unwieldy. With a table of squares and square-roots and a good book of computation-tables at hand, the four equations with numerical coefficients can be worked out with a little care and patience.

B and B' follow at once. Finally, by calculating the terms in the energy equation (3) and dividing throughout by $c\rho A^2$ we obtain numbers showing the partition of energy among the reflected and refracted waves.

When any one of the quantities c', Y, Y' becomes zero, or imaginary, there is no wave of that type. In such cases, the A and B quantities may work out in the form $a + b\sqrt{-1}$ and we must then take the expression $\sqrt{a^2 + b^2}$ as the number on which the energy depends.

In this same paper, Knott gives the results of a number of calculations for rock-air, rock-water, granite-slate, and other interfaces. His assumptions regarding the elastic constants, however, are too generalized to be of value in the present instance except as qualitative indications of the complexities encountered.

Accordingly, there is submitted here a numerical computation for specific rock-types with constants and velocities actually determined by experiment, of a magnitude similar to those encountered in the field.

Assume a Mesozoic sandstone in contact at a plane-interface with a Permian limestone. Let the symbols assigned to the sandstone be designated by plain letters, and those for the limestone by the same letters primed. Let ρ be the density, v the longitudinal velocity, u the transverse velocity, n the rigidity, and k the bulk-modulus, or incompressibility. Poisson's ratio, σ, is given, as well as the ratio v/u which has a value of 1.73 when σ is 0.25. It will be noted that k' is more than twice k. Evidence of the qualitative reasonableness of this can be found in the International Critical Tables, Volume III, where the compressibility of a limestone at 300 megabaryes is given as 2, and that of a sandstone at 140 megabaryes as 8. The notable change in compressibility for these rocks between 1 and about 2,000 megabaryes makes a value in the neighborhood of 4 appear entirely feasible for sandstone at 300 megabaryes. Thus, a compressibility for limestone of less than half that of sandstone confirms an incompressibility for limestone of more than twice that of sandstone assumed here.

It should be kept in mind in computing velocities from constants determined in the laboratory by "static" forces that the "dynamic" values of moduli are sometimes as much as 25 per cent greater than the "static" values. Since it is relative rather than absolute velocities that concern us here, however, no account will be taken of this fact.

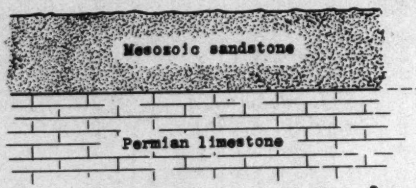
It was found in carrying through the computations that at least four, and preferably five significant figures need to be retained up to the final results.

As a practical matter, the various angles of reflection and refraction can be more easily calculated by the ordinary sine law than by equation (1). It should be noted that the surface-trace is common to all the waves. In other words, each wave velocity is, so to speak, the component in its direction of the velocity of propagation of the surface-trace.

Let a condensational wave be incident at an angle θ to the boundary surface separating two media, and let v, v' be the speeds of propagation of the

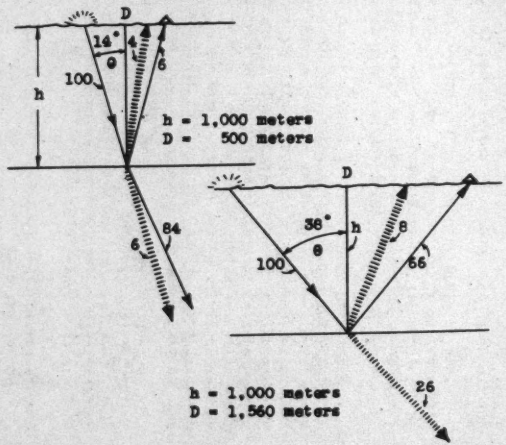
PARTITION OF ENERGY IN REFLECTED AND REFRACTED RAYS

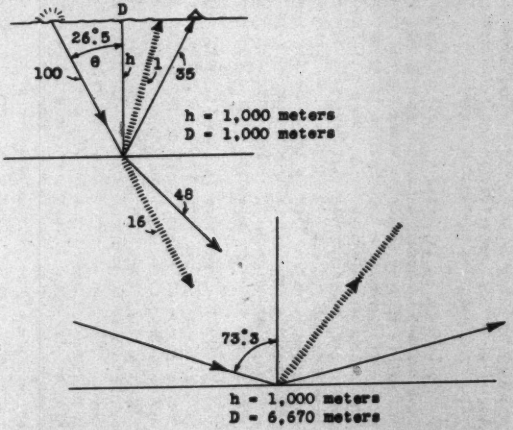
Condensational wave in sandstone incident on limestone interface



$\rho = 2.30$	$u = 1.950$ km/sec	$m = 14.23 \times 10^{10}$
$v = 3.160$ km/sec	$v/u = 1.65$	$n = 8.74 \times 10^{10}$ cgs
	$\sigma = 0.19$	$k = 11.32 \times 10^{10}$
$\rho' = 2.65$	$u' = 3.370$ km/sec	$m' = 39.74$
$v' = 5.130$ km/sec	$v'/u' = 1.52$	$n' = 30.11 \times 10^{10}$ cgs
	$\sigma = 0.12$	$k' = 29.71 \times 10^{10}$

Where $v^2 = (k + 4n/3)/\rho$; $u^2 = n/\rho$; n = rigidity; k = incompressibility; m = $(k + n/3)$





condensational waves. Let u and u' be the speeds of the distortional waves. The corresponding angle θ' for the refracted condensational wave, and the angles ϕ, ϕ' for the reflected and refracted distortional waves, are given by $v/\sin\theta = v'/\sin\theta' = u/\sin\phi = u'/\sin\phi'$.

Now, as u is less than v, there will always be a reflected distortional wave, except at normal incidence when $\theta = 0°$ and at grazing incidence when $\theta = 90°$. There will be refracted waves for all except the limiting incidences, if $v > v'$. If v should be intermediate in value to v' and u', there will be no refracted condensational wave for angles of incidence higher than a certain critical angle; the condensational wave will be totally reflected. If v should be less than both v' and u', there would be special angles of total reflection for both kinds of waves. This is the situation in the numerical calculation following. When the critical value corresponding to the refracted distortional wave is reached, there will be total reflection of both types of waves, and the whole energy of the incident wave will be divided between the two reflected waves.

If the incident wave is distortional, there must always be a critical angle of incidence for and above which the reflected condensational wave vanishes. The existence of such critical angles for the refracted waves will depend upon the relative values of u, v', u'--the condition for the possibility of total reflection being that v is less than v'. An interpretation of the results can best be given by means of a series of diagrams for the various angles of incidence. The work of determining the relative energy partition was not carried beyond the critical angle for the condensational-rarefactional waves. Computation to that point illustrates the principle with sufficient clearness, and, further, emphasizes the fallacy inherent in rectilinear propagation formulas which postulate refraction of waves along the surface of the second medium at the critical angle. It will be seen here mathematically demonstrated according to classical theories of elasticity that such a situation is impossible physically. Recent work by Uller casts doubt on certain aspects of these results, but the degree to which they are affected is not yet clear.

After a glance at the results, comment on their significance is scarcely necessary. It is obvious, of course, that the distance between shot and recorder at which the various rays would be picked up is a function of the depth to the limestone-formation. An indication of what this means quantitatively is given by showing the surface distances resulting from a depth of 1,000 meters.

Perhaps the most striking feature is the difficulty, if not impossibility, of relying for any two given media upon intuition to forecast the behavior of the waves. From the standpoint of refraction-shooting, the effect of a sloping interface is fundamental, in connection with the amount of energy from an incident ray which goes into the refracted rays. In reflection-work, as well, anyone with optical analogies thoroughly ingrained would probably be surprised to note the possible distances within which the portion of the incident ray which is reflected carries so little of the energy. Here, also, is further evidence of the need for recording components of motion which will permit the identification of longitudinal and transverse waves, as well as angles of emergence.

It is probable that wrapped up in these few equations are facts which would go far toward explaining anomalous shifts in recorded energy along certain profiles, failure to get reflections at one place, success in another, and many other uncertainties, admitted or unrecognized, in the problem of interpreting vibrations from dynamite blasts.

Harvard Seismograph Station,
Cambridge, Mass.

SECTION OF METEOROLOGY

(Washington, D. C., April 26, 1929)

REPORT OF THE METEOROLOGICAL DIVISION OF THE COMMITTEE ON THE PHYSICS OF THE EARTH OF THE NATIONAL RESEARCH COUNCIL

INTRODUCTION

H. H. Kimball

(Abstract)

The bulletin bearing the title "The Physics of the Earth--Meteorology" being prepared for publication under the auspices of the Division of Physical Sciences of the National Research Council will contain six chapters: (1) The origin and composition of the atmosphere; (2) meteorological records and climatic variability; (3) solar radiation and its role; (4) the meteorology of the upper air; (5) the thermodynamics of the atmosphere; (6) the physical basis of weather-forecasting.

From the beginning animal life has been greatly influenced by the weather. Man early began to talk about it, as is evidenced by weather-proverbs and legends of great antiquity. The Greeks made the weather a subject of philosophical inquiry, and Aristotle's Meteorologia was the standard work on the subject for 2,000 years. Early in the seventeenth century the application of inductive methods to studies of the natural sciences and the invention of the thermometer and the barometer led to systematic measurements of meteorological elements and enabled Halley, Hadley, and others to attempt a physical explanation of weather-phenomena.

The invention of the electric telegraph made it possible to collect weather-reports quickly from large areas, from which to construct synoptic weather-maps. These proved so useful in weather-forecasting that national weather-services were quickly formed all over the world.

Since man has learned to navigate the air, it has become necessary to extend routine meteorological observations to heights at which airplanes fly. For theoretical purposes the spectroscope has been employed to study the air to its outmost limits. This instrument has also been made use of in studies of solar radiant-energy, which is recognized as the force that keeps the great atmospheric engine running.

Public demands for the application of meteorological data to commercial and industrial needs are at present taxing the resources of national weather-services. It is inevitable, however, that the more exacting requirements of interests now served will lead to increased attention to the development of theoretical meteorology, not only by national weather-services, but also by universities. And thus meteorology will take its rightful place as an important branch of the physical sciences.

U. S. Weather Bureau,
Washington, D. C.

THE ORIGIN AND COMPOSITION OF THE ATMOSPHERE

W. J. Humphreys

See report in this Volume presented at eleventh annual meeting of the Section, May 1, 1930.

METEOROLOGICAL DATA AND METEOROLOGICAL CHANGES

C. F. Marvin and A. J. Henry

(Abstract)

This report deals with climatic changes as read from the geologic record which shows among other things that the Earth has undergone at irregular intervals measured by thousands of years wide-spread glaciation with its attendant lowering of temperature and the destruction of all forms of life in the regions of most intense glaciation. The evidence of geology consists in the main of the distribution of glacial till throughout the world and the remains of fossil plants and animals.

The study of fine seasonably banded clay-deposits (varves) laid down in quiet lakes and fiords by retreating glaciers also has afforded much valuable material. From a study of such material Swedish geologists have placed the end of the glacial period in Sweden at 6,500 years B.C. From that time up to the beginning of the instrumental period about the middle of the nineteenth century the evidence of climatic change is more or less shadowy and uncertain; there is, nevertheless, indications of a very general dessication in parts of Asia in the last 2,000 years.

In comparatively recent years a study of the annual growth-rings of the western yellow pine in Arizona discloses a correlation with rainfall sufficiently close to warrant the inference that the width of the annual growth-rings may be used as an indication of the rainfall of the regions in which the trees grow.

It was not until about two centuries after the invention of the barometer and thermometer that their use became sufficiently general so as to afford observational material for the study of weather changes. Contrary to a somewhat general belief that instrumental records are available as early as the eighteenth century, the facts are that the number of observing stations are so few and so widely separated as to be practically negligible, until, as before stated, about the middle of the nineteenth century. There is, therefore, available for study not to exceed 100 years of observational material for but a small number of stations at widely separated points over the globe.

Several notable contributions have been made to the subject of investigation and charting of world weather and these have been preceded by mapping the weather of the globe or some part of it. The first attempt to chart the weather for both land and water surfaces dates back only 60 years, and it is only within a year or so, to be exact, in August 1927, that a collection of printed summaries of monthly mean pressures and temperatures and total amounts of rainfall for all places having long records thereof throughout the globe became available. The printing of this record was possible through the generosity of an American citizen--Mr. John A. Roebling.

Hildebrandsson and De Bort (1907) made an important contribution to world meteorology and various investigators since that time have added to our knowledge on the subject.

In quite recent years, the eighties and nineties of the nineteenth century, notable contributions have been made to our knowledge of the meteorology of the free-air by means of kites and manned balloons carrying registering instruments. At present, in order to meet the needs of the aviator, observations of the movement of air-currents are made through the instrumentality of the so-called pilot balloon. Practically all meteorological services of the world include in their daily program observations of this sort in the interest of aviation.

Recent years have also seen the beginning of observations on solar variability by the Astrophysical Observatory of the Smithsonian Institution, that Observatory now maintaining three stations at widely separated places for the observation of changes in the solar-constant of radiation.

(In connection with this paper, see also report in this Volume presented at the eleventh annual meeting of the Section, May 1, 1930.)

U. S. Weather Bureau,
Washington, D. C.

SOLAR RADIATION AND ITS ROLE

H. H. Kimball

METEOROLOGY OF THE FREE ATMOSPHERE

W. R. Gregg

DYNAMIC METEOROLOGY

Edgar W. Woolard and Hurd C. Willett

PHYSICAL BASIS OF WEATHER-FORECASTING

C. G. Rossby and R. H. Weightman

For brief abstracts of these reports presented informally at the tenth annual meeting, see the summary given in this Volume of the report presented at the eleventh annual meeting, May 1, 1930, on the Bulletin of the Division of Physical Sciences of the National Research Council on Meteorology.

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SECTION OF TERRESTRIAL MAGNETISM AND ELECTRICITY

(Washington, D. C., April 26, 1929)

SYMPOSIUM ON PHYSICAL THEORIES OF MAGNETIC AND ELECTRIC PHENOMENA

THE CORPUSCULAR-RAY THEORY OF THE AURORA

N. H. Heck

The discussion of the corpuscular-ray theory of the aurora has been inspired from two sources. The first is the excellent summary by Dr. Carl Störmer in the magazine Arktis in which, in anticipation of important attack on the auroral problem through exploration of the arctic by means of an airship, he sets forth the progress that has been made in the investigation of the aurora and the outstanding problems that remain. The second is the interest recently shown in the United States in the development of new theories or modification of old ones. The work done in the past has been fully published, but the publication has been scattered through many volumes, theories have been advanced, accepted for a time, and then rejected; different theories in regard to the same phenomena are held. The result has been a state of confusion in the minds of many as to just what progress has been made. If my presentation can help, even in part to clear up this confusion, the effort will not have been in vain.

While work has been done on the aurora in a number of countries and observations have been made in many places both in the arctic and antarctic regions and in middle latitudes, the fact remains that the home of this study has been in Norway. This could have been expected in a country containing highly intelligent people with abundant opportunities to observe the phenomena, but this does not detract from the credit for the very great accomplishment. There are three outstanding names, Störmer, Birkeland, and Vegard, but many others have contributed to the study.

The history of auroral investigation has in some respects followed that of terrestrial magnetism. Just as no important progress was made in the latter subject until accurate methods and instruments had been devised and put into use continuously and systematically in many parts of the Earth, so no real progress was made in auroral study until means had been found to supplement the inaccurate visual methods with more accurate and permanent records.

Störmer found methods available that were capable of solving this problem, but to him belongs the credit of overcoming difficulties and perfecting methods so that the aurora could not only be photographed but its height determined. The difficulties are far in excess of those involved in the not-too-easy astronomical photography, since the aurora, while it appears bright on a dark night, is relatively feeble, its light characteristics are such as to reproduce poorly, and many of the phenomena are so fast-moving that their light is not sufficient to permit the short exposures required by motion-picture photography. The method of determining the height is to photograph the aurora against stars of the first magnitude simultaneously at two or more stations, the camera being directed at the same stars and exposed at the same instant by means of telephone communication. The parallax or displacement with respect to the stars of the salient features of the aurora gives the necessary data for computing the position and height. Thousands of such observations have been made and there are at hand many valuable records for auroral study.

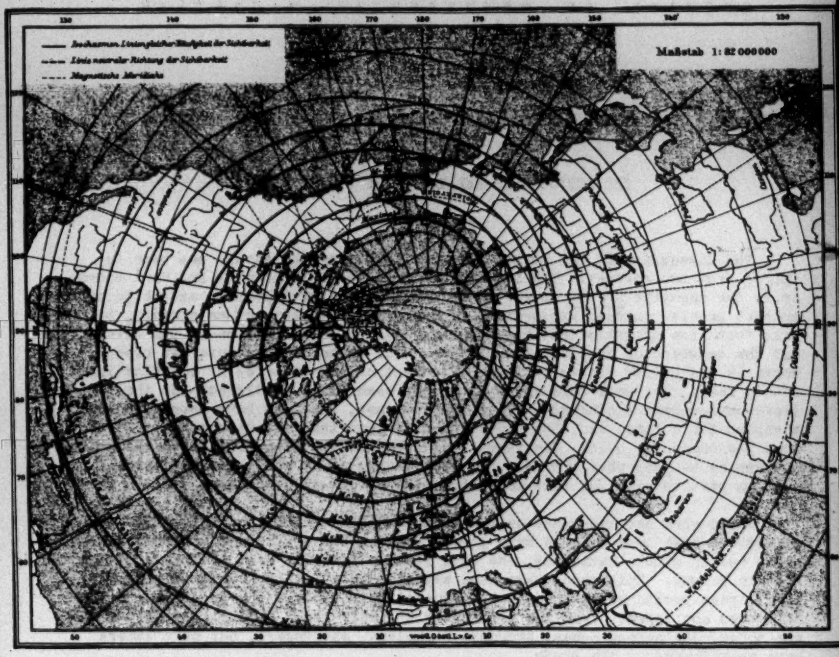

Fig. 1--Isochasms or lines of equal auroral-frequency (after H. Fritz)

Before reviewing some of the theories which have been advanced to explain the aurora, a brief review of some of the principal facts is desirable. Vegard has listed the principal types of aurora as (1) quiet forms and (2) moving forms. Classified as quiet forms are: Auroral haze or cloud; pulsating rays; quiet arcs; stationary pulsating bands (alternations generally proceed from west to east). The moving forms include: Arcs with sharp contours (light-waves move along the arcs from west to east, color green-yellow and at times deep red); draperies (with under edges very sharp); rays, either isolated or in bundles (these may extend to much greater heights than those forming draperies and may occur simultaneously with other forms); corona (mixture of draperies and rays passing the meridian near the zenith). Many of these forms have definite radiation-points which fix the direction of the rays. In addition to the principal forms which have been listed, which probably vary very little physically, there are a number of transformation-forms.

The subject was first studied from the statistical viewpoint and it soon became evident that lines of equal frequency are nearly circular in form (see Fig. 1) and in 1807 Finley first mapped these curves which he called isochasms. The one representing the maximum frequency which is actually a belt of some

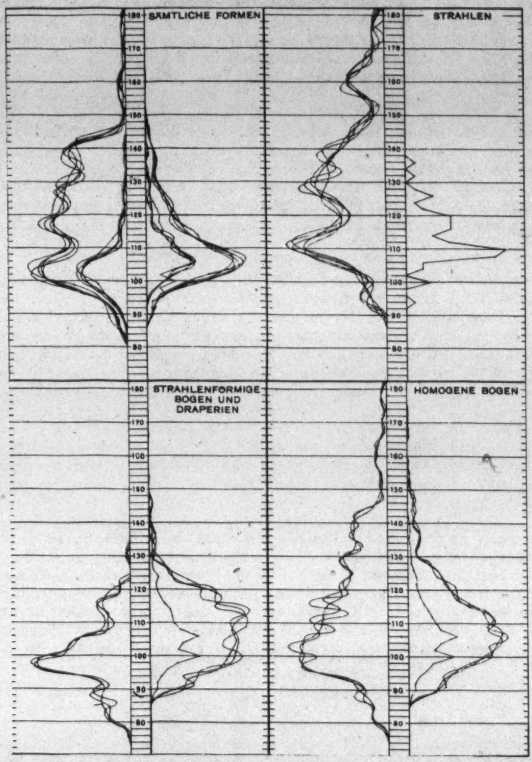

Fig. 2--Relative frequencies and minimum heights in kilometers of auroral lights of different types after Störmer for southern (to left) and northern (to right) Norway

width is known as the belt of maximum auroral-frequency and will for convenience hereafter be called the auroral belt. This lies about 23° from its center which may be called the auroral pole. The existence of this belt in this position is one of the most important facts that all theories on the aurora must explain.

The height of the auroral forms has an important bearing on any theory that may be adopted. All theories require some sort of penetration that has the power of rendering the atmosphere luminous, and the lower limit of the forms represents the penetration. The height of the lower limit attained by various forms of aurora is therefore of importance. In general this is about

100 kilometers. Out of 1927 measurements the average was 108 with variations from 105 to 113 kilometers. The variation with form of aurora is not very great and the variation with latitude does not appear to be more than two to four per cent. In the auroral belt the upper limit is rarely more than 300 to 350 kilometers while in lower latitudes heights of 800 and even 1,000 have been observed.

The radiation-point has been determined for a number of forms and it is found that it nearly agrees with the magnetic zenith, that is, the place where the tangent to the magnetic line of force passing through the place intersects the celestial sphere. In fact it is systematically nearly a degree lower, possibly indicating that the direction of the line of force is different at the height of the aurora from what it is at the surface of the Earth where it is measured.

The variation of frequency during the day is of interest. One question is whether the auroras group themselves symmetrically around a suitable time of reference. The time used for this purpose is magnetic midnight, which is defined as the time at night when the place concerned is in the plane of the Sun and the magnetic axis of the Earth. The grouping is found to be definitely unsymmetrical and the daily maximum is about one hour before magnetic midnight or, for the United States and Canada, between 9 and 9 p.m. local time.

The auroras which are seen in middle latitudes are invariably accompanied by magnetic storms, while those occurring only in the auroral belt may or may not be. For this reason the auroras of middle latitudes may have 27-day periods and do have 11-year periods, while these periods are not so evident in the auroral belt.

There are many other facts which might be mentioned but these suffice to give some picture of the facts which acceptable theories must explain. Obviously the phenomena on which the most exact observations can be made are a suitable point of attack, so Birkeland and Störmer selected the auroral arc. If it can be shown that rays of a known type in the form of a narrow elongated pencil or bundle can penetrate the Earth's atmosphere and excite luminosity through the observed auroral range, distinct progress would be made.

Fig. 3--Ring resembling aurora produced on terella by cathode-ray bombardment (after Birkeland)

Birkeland advanced the theory that a circular pencil of corpuscular rays is shot out from the Sun, possibly through a narrow aperture, and traverses space until it reaches the Earth's atmosphere and magnetic field. The forces acting distort the shape of the pencil from a circular to an enormously elongated one with its axis along the auroral belt.

The individual rays follow the lines of force of the Earth's magnetic field until stopped. The stopping is caused by reduction of velocity and by absorption so that the rays are no longer able to produce visible radiation. The possible paths taken by the rays depend on their character, their initial velocity and their deflectability.

The possible paths are so numerous that Birkeland undertook by laboratory experiment to get some light on the range of possibilities. He bombarded by cathode-rays a magnetized globe, which he called a terella, and produced approximations of some of the auroral effects. Among the thousands of experiments were several by which he produced luminous polar rings resembling the auroral belts and also an equatorial belt which will again be referred to as well as spirals and other forms (see Fig. 3). He demonstrated that the aurora may be seen when the Earth intervenes between the Sun and the place of observation.

Störmer and Birkeland then supplemented the results obtained with the terella by mathematical analysis. Their monumental task was to study possible trajectories of the rays. Störmer, to bring the problem within range of attack, assumed that the Earth's magnetic field is equivalent to that of an elementary magnet at its center with its axis along the Earth's magnetic axis, which makes an angle of 11° with the true axis, and the northern end of which lies in Smiths Sound, Greenland, which is approximately the center of the auroral belt, that is, the auroral pole.

A special effort was made to find trajectories which, coming from an infinite distance, would reach the origin (in this case the center of the Earth, a purely mathematical conception). Such rays would penetrate furthest into the Earth's atmosphere. He defined the tangent to the direction with which such rays would leave the Sun as a distinguished or critical direction. Rays departing even slightly from this direction may spiral along the Earth's magnetic lines of force and penetrate to some depth and others would circle around the Earth several times, in some cases coming close enough to excite luminosity but only after passing around several times. The portion of the rays which become luminous is that portion of the spiral paths where the pitch of the spiral is comparatively small.

Vegard in 1911 developed a strong argument in favor of the possibility of the rays being alpha-rays. These start with a definite velocity and if they strike a homogeneous layer of matter they penetrate with nearly straight-line orbits and with little scattering. He claimed that they would penetrate sufficiently to account for the aurora but others have found that they would not come closer than 250 kilometers. Besides the auroral belt would be 16° to 18° from the magnetic axis instead of 23°.

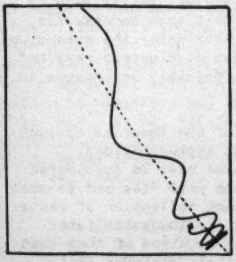

Fig. 4--Trajectories of electric corpuscles (after Störmer)

Whatever type of rays is adopted this discrepancy exists in greater or lesser degree and the phenomena are too definite to permit of any such discrepancy in a permissible theory.

Störmer recognized this and, calling to mind the equatorial ring of the terella, modified the whole theory by adding an equatorial current-ring and was able not only to bring the auroral belt to the proper place and keep it there regardless of the

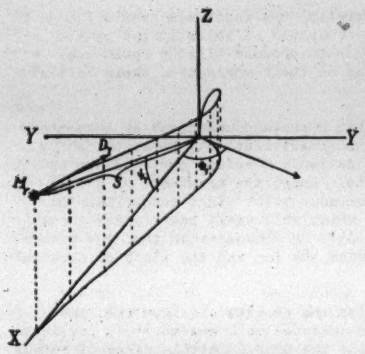

Fig. 5--Complex trajectories of electric corpuscles (after Störmer)

type and character of aurora but was able to account for auroras reaching to the middle latitudes when the currents in the ring are exceptionally strong. He considers the ring as set up by rays arriving from the Sun and maintained by continuously arriving rays. He meets the objection that such a ring would set up disturbances on the Earth's surface in excess of those measured by showing that the phenomena could be accounted for without any change greater than 30 gammas in the observed intensity.

In arriving at these conclusions, Störmer admits that he has neglected certain factors which may not be negligible and does not consider the problem as finally solved.

In order that any theory may be satisfactory, it is necessary to have some idea of the composition and physical condition of the upper atmosphere. It was first thought that it is composed of hydrogen and helium but the spectroscope showed that this could not be the case. The characteristic green-line of the aurora was difficult to obtain and Vegard first secured it by bombarding solid nitrogen with cathode-rays. He then advanced the theory that the upper atmosphere must contain small crystals of nitrogen. McLennon obtained the lines from mixtures of oxygen and helium, and it is now generally accepted that the upper atmosphere contains the same elements as the lower though probably in somewhat different percentage.

The fact that the characteristic green-line of the aurora is found practically everywhere in the night sky is probably corroborative.

The temperature of the upper air was deduced by Störmer from auroral considerations but other considerations are now given greater weight.

Störmer has found that at times an aurora of very unusual height such as nearly 1,000 kilometers has been observed and he finds that such auroras lie high enough above the Earth to be in the sunlight. He attributes the extension of visibility to this great height to the combined effect of electric rays and ionization of the upper atmosphere by sunlight and also possibly to changes in temperature.

Careful consideration seems to indicate that none of the theories account for all the phenomena though some of them serve as first approximations. Störmer considers that all that has been done to this time must be considered pioneer work. However, the important contribution of the past lies not so much in the theories advanced as in the careful and painstaking collection of facts and in the careful and complete testing of theories. It is now established that those advancing new theories must expect to have it required of them that their theories should meet the test of explaining accurately observed facts.

There is every prospect of renewed attack on these problems. A large amount of observational data was obtained by Sverdrup on the Maud Expedition.

In addition to the work of this and other expeditions, there are in existence or in prospect new permanent stations for the observation of the aurora. The Rockefeller Foundation has made it possible to establish a laboratory in Norway, its prime purpose being the study of the aurora. Observations are being carried on near Fairbanks, Alaska, at the Alaska Agricultural College and School of Mines. The International Union of Geodesy and Geophysics at its Prague meeting in 1927 set up an international committee on the subject of which Störmer is chairman and the North American representatives are Sir Frederic Stupart of Canada and N. H. Heck of the United States.

Many problems remain to be solved. Among those listed by Störmer in the article to which reference was made are included as of special importance the following: Study of the passage of various kinds of rays through rarefied gases in order to reproduce the aurora as closely as possible; the investigation of different types of aurora occurring simultaneously, especially simultaneous arcs of different elevation; the determination of the maximum length of an auroral arc (one has been found to extend 1,500 to 2,000 kilometers but its full length was not known); the investigation of extraordinarily high or low aurora; photography of fast-changing forms by improved motion-picture apparatus and methods; spectroscopy of changing forms; and explanation of different types of variations in same type. These and an almost infinite number of other problems leave a wide field for future study.

U. S. Coast and Geodetic Survey,
Washington, D. C.

THE ULTRAVIOLET-LIGHT THEORY OF AURORA AND MAGNETIC STORMS

E. O. Hulburt

(Abstract)

The ultra-violet light theory of aurora and magnetic storms suggests: (1) That because of winds and unusual production of ions the ionized region of the high atmosphere may be in violent agitation during the day of a magnetic storm that it grows calmer at night and becomes agitated the next day if the storm continues; (2) that there may be auroral displays in polar regions with no magnetic storms in temperate latitudes; (3) that auroral displays in temperate latitudes should as a rule be accompanied by magnetic storms; and (4) that polar aurora should follow several hours or a day after strong magnetic disturbances observed in temperate latitudes. These inferences are shown to be in agreement with the data from magnetic observatories and observations of aurora in temperate and polar latitudes. (The complete paper is published in Phys. Rev., v. 34, 344-351, 1929.)

U. S. Naval Research Laboratory,
Washington, D. C.

OZONE: IS IT DUE TO CORPUSCULAR OR ULTRAVIOLET-EMISSION FROM THE SUN, OR BOTH?[1]

F. E. Fowle

[1] Published with the permission of the Secretary of the Smithsonian Institution, Washington, D. C.

Considerable evidence is accumulating leading to the supposition of two electrically conducting-layers in the upper atmosphere. The writer wishes to indicate that the observations treated in this paper are not inconsistent with such a supposition.

These observations lead to measures of the amount of energy absorbed from the incoming solar radiation by the Chappuis band, due to atmospheric ozone, in the yellow region (0.4500 to 0.6500μ) of the spectrum. They are used to indicate what changes may take place from time to time in the amount of this ozone. For details of the measurements the reader is referred to the writer's paper in the Smithsonian Miscellaneous Collections, volume 81, 1929 (a preliminary report appeared in Terr. Mag., v. 33, 151, 1928).

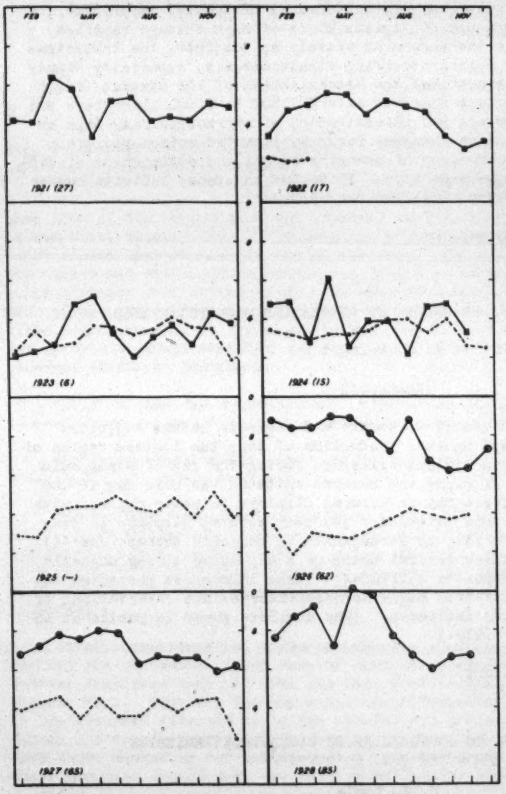

Fig. 1--Monthly means (unit, 0.001 cal/cm^{2}min) of measures of ozone-absorption 1921-27

The monthly means of these measures for eight successive years will be found plotted in the eight sections of Figure 1. The abscissae (months) and ordinates (ozone-absorptions) are plotted similarly in all respects as to scale in the eight sections. The successive years may thus be directly compared. The values connected with heavy continuous lines come from Harqua Hala (latitude 34° north), 1921-23, and Table Mountain (latitude 34° north), 1924-28; those connected by lighter broken lines were made at Montezuma (latitude 22° south) except that in 1922 the station was at Calama. The year for each section is indicated, followed by the mean Wolfer sunspot-number, averaged for the days on which ozone was measured. These numbers relate specifically to the days corresponding to the northern stations; hence no spot-number appears after 1925 during which year no northern observations were available.

A comparison of the observations plotted in these eight sections leads to several quite definite relationships.

Consider the dotted lines representing the stations in the southern hemisphere. First, it is to be noted that the lines remain at nearly the same height in each year's compartment; next, the range of the values is comparatively small; and, finally, they show a definite annual march--the maxima coming between August and September and the minima between April and May.

Next taking in hand the heavy continuous lines due to the stations of the northern hemisphere, we may draw considerably different conclusions: First, the height varies greatly in the successive sections; next, this change in height keeps step with the sunspot-number of the section--the height being high with large spot-number, low with small number; further, at sunspot-minimum, the height coincides with that of the southern-hemisphere station; and, finally, the yearly march, though still present, shows maxima and minima at reversed months from the other stations, although in each case in the same season of the year.

Turning to Figures 2 and 3, we find the relationships with the sunspot-numbers even more clearly and definitely indicated. It is decidedly close with the values from the northern stations whereas no relationship is indicated from the southern stations. The writer does not consider the conclusion as to sunspot-correlation from the southern stations of very much weight. With small amounts of ozone the accuracy of the points is small. The points for the northern stations rest upon observations for 89, 57, 37, 36, 211, 58, and 204 days, respectively.

Next a word as to the annual march with the maxima and minima. Because of its yearly period it should be associated with some change in the position of the Earth in its annual orbit about the Sun. There are three unique pairs of position: (1) The most northerly and southerly declinations of the Sun, June 21 and December 21, respectively; (2) the Earth in perehelion and aphelion, January 4 and July 4, respectively; and (3) the greatest northerly and southerly displacements of the Sun's equator as seen from the Earth, September 8 and March 7, respectively. The latter positions agree most nearly with the maxima and minima of the observed annual march. A further discussion of this matter is given in the Smithsonian publication above cited. If this association is veridical, it indicates the Sun as the cause of this ozone.

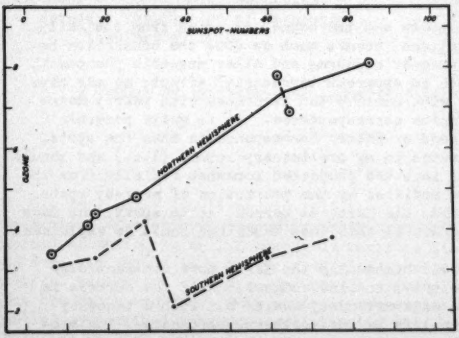

Fig. 2--Comparison ozone-absorption in northern and southern hemispheres with sunspot-numbers

It does not seem unlikely to the writer that the following explanation may fit the above results: Under the influence of the Sun's ultraviolet light which shines, let us say, nearly equally on both hemispheres, except as

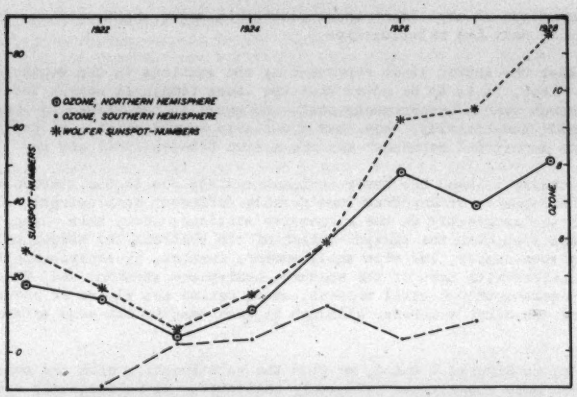

Fig. 3--Graphs of annual means for ozone-absorption and sunspot-numbers in northern and southern hemispheres 1921-28

modified by the Earth's position in its orbit, there is formed a somewhat basic amount of ozone. The range of this amount may be limited by the known destructive effect upon the ozone of the longer wave-lengths in the Sun's spectrum. It has its maximum and minimum values at reciprocal months though at the same seasons in the stations of the two hemispheres.

Besides this layer depending on the general ultraviolet light from the Sun's surface, the writer wishes to postulate another layer very closely connected with the sunspot-cycle and possibly due to some corpuscular emission from the Sun. This corpuscular emission may be in the form of ions, emitted from hotter portions of the Sun's disk, possibly from the flocculi.

The connection between the spots and the ozone, as noted from the daily observations at the northern stations, occurs much as does the connection between the spots and terrestrial-magnetic storms and other magnetic phenomena; spots may appear on the disc with no apparent terrestrial effect; we may have an effect without them; and yet with monthly and even more with yearly means there exists an exceedingly definite correspondence. It is quite possible that the flocculi will show as good or better correspondence than the spots. Some such relationship was indicated in my preliminary report (l.c.) and should be followed up. If the required ions are projected somewhat radially from the bright flocculi and their course modified by the attraction of near-by spots, so that the aim of the ions towards the Earth is marred, so to speak, and made less accurate, then some such effect as that just described could be explained.

Is the effect, due to the ions bombarding the Earth more dominant over the northern hemisphere? I have tried to find comparisons of the aurorae in the two hemispheres but find the data extremely meager but with a tendency towards indicating a greater intensity in the northern hemisphere. The measures of Rayleigh on the auroral green-line (due to atomic oxygen) in the night sky seem to give a slightly greater average intensity for the northern skies.

Finally a word as to the height of these layers and the heights inferred from other phenomena: (1) From radio reflection-results, heights are generally taken at about 105 and 225 kilometers; (2) from aurora heights, 85 to over 300 kilometers, with well-defined maxima at 100 and 106 kilometers; (3) for magnetic phenomena, Chapman infers a diurnal disturbance-layer, variation-layer due to ultraviolet light at 10 to 90 kilometers, and a magnetic disturbance-layer due to ions from the Sun at 90 to 100 kilometers; (4) Cabannes obtained for the height of the ozone, 40 to 50 kilometers, and Dr. Dobson, 30 to 40 kilometers. Probably some of the above figures should be modified.

Astrophysical Observatory,
Smithsonian Institution,
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THE ATMOSPHERIC DYNAMO-THEORY OF VARIATIONS IN EARTH-CURRENTS AND TERRESTRIAL MAGNETISM--A REVIEW

O. H. Gish

(Abstract)

The atmospheric dynamo-theory maintains: That the atmosphere at a height from 100 to 170 km is a relatively good conductor, equivalent to a sheet of copper one-tenth mm thick directly under the Sun; that motion of this relative to the Earth's permanent magnetic field due to winds arising from tidal and other forces generates electric currents in this region of the atmosphere; that the magnetic field of these primary currents together with that of the secondary currents induced in the Earth by variations in the former constitutes the observed fluctuations of the Earth's magnetism, except those of very long period (secular variations) and some short-period fluctuations (local disturbances) which appear only over small areas. This theory was briefly outlined and its tenability considered.

Department of Terrestrial Magnetism,
Carnegie Institution of Washington,
Washington, D. C.

A THEORY OF THE PERMANENT MAGNETIC FIELD OF THE SUN AND EARTH

Ross Gunn

(Abstract)

In this paper (published in full in Phys. Rev., v. 34, 335-343, 1929) the motions of electrons executing short free-paths under the influence of thermal agitation in an inhomogeneous magnetic field or in crossed electric and magnetic fields is shown to give rise to electric current-systems. Gravitational separation of charge produces a symmetrical radial electric field inside the Earth and Sun. This radially symmetrical electric field and a small initial symmetrical inhomogeneous magnetic field give rise to current-systems which are in such a direction as to increase the original magnetic field and regeneration takes place. The field is limited by some mechanism which is not understood. The permanent fields arise from the thermal energy of the body and would be maintained even if the bodies ceased their rotation. In order to

account quantitatively for the observed current-system it is necessary that the electron mean free-path inside the Earth approximate 10^{-7} cm. The theory emphasizes the necessity for a better understanding of electrical conduction in solids; in particular a more satisfactory theory of the Hall effect is desirable.

U. S. Naval Research Laboratory,
Washington, D. C.

THE RELATION OF RADIO-WAVE PROPAGATION TO DISTURBANCES IN TERRESTRIAL MAGNETISM

I. J. Wymore

(Abstract)

From a study of the variation in long-wave daylight signal-intensity observed at the Laboratory for Special Radio Transmission Research, Bureau of Standards, and of the disturbances in terrestrial magnetism, it is concluded that there is a marked increase in the intensity of long-wave signals following severe magnetic storms. In the case of long-distance reception from European stations, there is a general tendency for the signal-intensity to be below normal for several days before the maximum of the magnetic disturbance and to show a definite increase from one to three days after the passage of the storm. Observations taken on stations at moderate distances, that is, the Radio Corporation stations at Tuckerton and New Brunswick, New Jersey, and Rocky Point, Long Island, give somewhat different results. While the intensity of signal from these stations is high from two to four days after the storm, as in the case of the distant stations, there is also a decided increase in signal-strength from two to four days before the magnetic disturbance. The complete paper is published elsewhere (Bur. Stan. J. Res., v. 2, 1201-1211, 1929; Inst. Radio Eng., v. 17, 1206-1213, 1929).

U. S. Bureau of Standards,
Washington, D. C.

ECHO-SOUNDING OF THE KENNELLY-HEAVISIDE LAYER

M. A. Tuve

(Abstract)

Observations of the Kennelly-Heaviside layer have been continued at the Department of Terrestrial Magnetism of the Carnegie Institution of Washington by the writer and L. R. Hafstad. The United States Naval Research Laboratory, as before, has kindly transmitted the necessary schedules of signals. Multivibrator-modulation was used, giving very sharp pulse-signals on 4,435 and 8,870 kilocycles. A marked diurnal-variation in the effective height of the layer and in the echo-pattern received for each transmitted pulse or "peak" was observed in three 24-hour series of observations taken during the early fall and winter. The echo-pattern shows multiplicities during the day and evening but becomes very complex at night. A few observations made during the magnetic disturbance of October 17 to 19, 1928, showed an unusually great effective height and a change in the echo-pattern.

Following a suggestion of G. Breit, an experimental determination of the rate of change of radio-frequency phase of the separate downcoming echoes has been carried out. The small power crystal-controlled oscillator circuit of the 4,435-kilocycle transmitter operates continuously, the high-power pulse-transmission being produced by modulating the power-amplifier circuits. A receiver was operated very near to the transmitter, having very rapid recovery from the paralyzing effect of the pulse ground-wave and sufficient sensitivity to receive the echoes with good amplitude, and having a very slight coupling to the crystal oscillator-circuit such that the crystal "pick-up" was comparable to the echoes received. The echoes showed their changing phase by alternately adding to and subtracting from the constant crystal oscillator pick-up. The phase-changes are regular, but the time of one 360° phase-change on 4,435 kilocycles varies from one second to 60 seconds during the day and evening and at times changes between these limits in as short a time as 15 minutes. When multiple echoes are present, the second and third echoes phase in and out more rapidly than the first echo, but not by an even factor.

Department of Terrestrial Magnetism,
Carnegie Institution of Washington,
Washington, D. C.

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SECTION OF OCEANOGRAPHY

(Washington, D. C., April 25, 1929)

OCEANOGRAPHY AND METEOROLOGY

Charles F. Brooks

(Abstract)

The sciences of oceanography and meteorology are more closely interrelated than the oceanographer or the meteorologist usually realize in the daily round of their work. The two fluid mediums, the ocean and the atmosphere, differ in composition, density, and viscosity, but obey the same laws of motion under differences of pressure, the deflective effect of the Earth's rotation, and friction. Both are heated by the Sun; both are cooled by radiation, also by conduction and evaporation. The interactions between the atmosphere and the ocean are intimate: The ocean, dense, slow-moving, conservative, profoundly affects the light air in contact with it; the atmosphere, light, mobile, radical in its temperature changes, quietly or boisterously pushes the ocean, stirring it and changing its density. The temperature, humidity, and wind-belts of the atmosphere and the temperature, salinity, and current-belts of the ocean have much in common and much interdependence.

The effect of warm and cold ocean-currents on climate is proverbial. But the more elusive dependence of storm-intensity on ocean surface-temperatures has still to be worked out fully. Obscure also is the full effect of weather on the movements and temperatures of the ocean-surface. Even the mighty Gulf Stream in the confining Straits of Florida suffers notable changes in surface-temperature and surface-current during the passage of a single storm. For example, during the recent heavy storm on the Atlantic seaboard the sea surface-temperatures in the western half of the Straits of Florida off Miami fell 2° Fahrenheit, and the western edge of the Gulf Stream seems to have moved some five miles to the eastward in but 36 hours. The stirring that was responsible for this probably raised temperatures at moderate depths, thus favoring a conservation of the thermal freight of the Gulf Stream for greater outpouring into the atmosphere far out in the Atlantic Ocean.

Clark University,
Worcester, Mass.

OCEANOGRAPHY AND LITTORAL GEOLOGY

Douglas W. Johnson

This paper was presented informally and no manuscript or abstract was prepared.

OCEANOGRAPHIC OBSERVATIONS IN MONTEREY BAY, CALIFORNIA

Henry B. Bigelow

This paper was presented informally and no abstract was prepared.

RECENT WORK ON THE DYNAMIC OCEANOGRAPHY OF THE NORTH ATLANTIC

C. O. Iselin

The Gulf Stream is a fascinating subject for research, not only because it is such a long and powerful current, but also because it is so difficult to study. It almost seems at times as if the current, knowing that an attempt is being made to discover its secrets, resists man's efforts just enough to keep him baffled. After two summer's work we have, for one reason or another, been unable to get even one hydrographic section that is entirely satisfactory. Twice the conditions have been obviously abnormal and our sections have crossed warm water flooding in over the continental shelf. Twice stormy weather has forced us to abandon a section before we had quite crossed the current. Finally several times we struck bands of water running so swiftly that it was impossible to tell at what depths our temperatures had really come from.

When a current is running three or four knots at the surface and the water is nearly still at a depth of a thousand meters, the wire and the instruments on it behave just as if a station was being made in still water from a vessel moving at a speed of three or four knots. In other words, the comparatively still water under the Gulf Stream holds the lower end of the wire while the ship moves along with the surface-current. A great bow of unknown dimensions is thrown in the wire and if any of the thermometer frames do work, the readings have little value as there is no way of telling from what depth they come. (Work on the Carnegie has indicated the uncertainty may be eliminated by using pressure-thermometers at a few depths.--Ed.)

For accurate dynamic calculations it is necessary to extend observations down to still water and we made several sections before we found that in the case of the Gulf Stream you have to go deeper than 1,200 meters to reach a base-plane. By base-plane is meant a depth of water deep enough so that there is almost no current. Calculated velocities will then give the true surface-velocity and not just the velocity of the surface relative to some intermediate depth which has an unknown speed.

Nevertheless in spite of all these troubles, by combining our ten sections a fairly good idea of the general principles peculiar to the Gulf Stream can be obtained. The map of the lines of stations in Figure 1 shows the general region of our researches. We have made 120 stations on these ten different sections during the last two summers. Since we have been using small schooners, this represents a good deal more time and work than it might at first seem.

Section B (Fig. 2) made during the first ten days of July 1927, runs from Bermuda to the mouth of Delaware Bay and is perhaps our best effort to date. Except for the fact that the observations did not extend quite deep enough to reach still water, this section is very satisfactory. Before going further "Gulf Stream" should be defined. There has always been much confusion in the literature. Certainly the water which comes through the Straits of Florida is the Gulf Stream. Now if we restrict the term to mean only water that has the Straits of Florida characteristics we can get along very nicely. I will call the narrow band of intense current which runs along our Atlantic coast the Gulf Stream. As will be shown later, this does not necessarily mean the warmest surface-waters, nor any water in which Gulf weed may be found floating. After reaching the Grand Banks the current loses its intense narrow nature, and from then on may very well be called the North Atlantic Drift.

But to return to section B, it will be seen from Figure 2 there are three

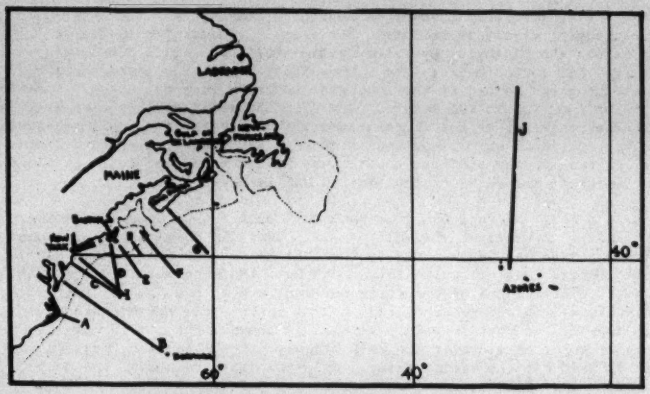

Fig. 1--Map showing lines of stations

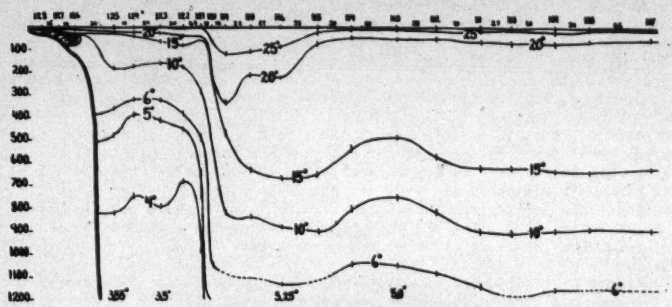

Fig. 2--Section B, temperature

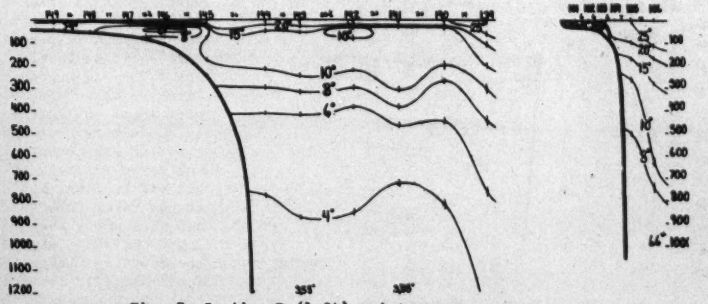

Fig. 3--Section D (left) and A, temperature

main divisions to the profile and that in each of these bands the water-conditions remain relatively uniform. For example, follow the 10-degree isotherm across the picture. Over the continental shelf it is found at a depth of from 160 to 180 meters--the intermediate band. The temperature-wall is very striking and beyond it the 10-degree isotherm runs out towards Bermuda at a depth of from 800 to 900 meters. Now this threefold division seems to be the typical arrangement of the water-masses off this coast and the three bands of water can be traced by our sections from Cape Hatteras to the Grand Banks. Moreover, in each of the profiles the various isotherms can be found at about the same depths as are shown on the section off Delaware Bay.

Section D (Fig. 3) runs out from Nantucket shoals in a direction roughly perpendicular to the axis of the Gulf Stream. Here the coastal band is found to be slightly wider as in this region the continental shelf is better developed; this profile does not quite extend out into the much warmer central Atlantic water. This is one of our first sections and we had not yet learned that the surface-temperature conditions do not surely tell whether or not the strongest current has been reached. As will be shown later, it is not the warmest water which constitutes the Gulf Stream, but the boundary between the intermediate band and the warmer water. Only the depth of one of the deeper isotherms is a sure indication of whether or not you have crossed the Gulf Stream. If we had continued this profile a few miles further offshore the isotherms probably would have dropped still further and then continued out horizontal at their new level. Notice the pool of relatively cool water that has been drifted out from the continental shelf. Such pools of cooler, fresher water are often found floating on the surface of the intermediate band and are, probably, the modern interpretation of the cold streaks which received so much publicity during the early days of the Gulf Stream's investigation.

Section G (Fig. 4), made in August 1927, runs out from the coast of Nova Scotia and shows that the same general features are present much further north. Of course, the water over the continental shelf is much colder but the intermediate band is again perfectly evident. As in the preceding figure, we did not continue this section far enough offshore, but the inshore half is shown as it is so typical. Just at the edge of the continental shelf can be seen a pool of cold water which is about to become detached.

Section J (Fig. 5), made in July 1928, runs due north from the Azores for 850 miles and thus crosses the major part of the North Atlantic Drift, as the continuation of the Gulf Stream is often called. On the right is an abrupt temperature-wall; as in Figure 1, the 10-degree isotherm again drops from about 100 meters to 800 meters, but instead of making just one abrupt drop as it did off Delaware Bay, it now makes three separate drops that appear as steps across the profile, rising again slightly between each of the steps.

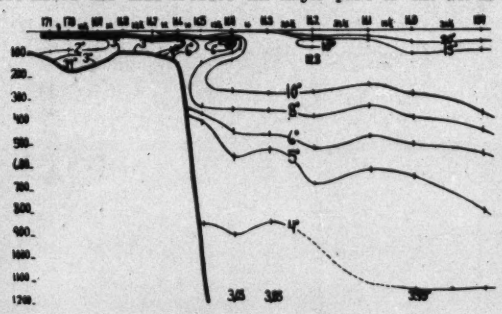

Fig. 4--Section C, temperature

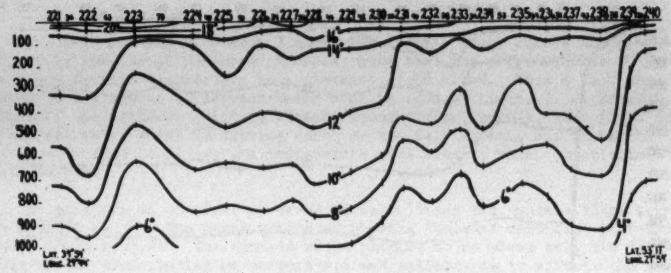

Fig. 5--Section J, temperature

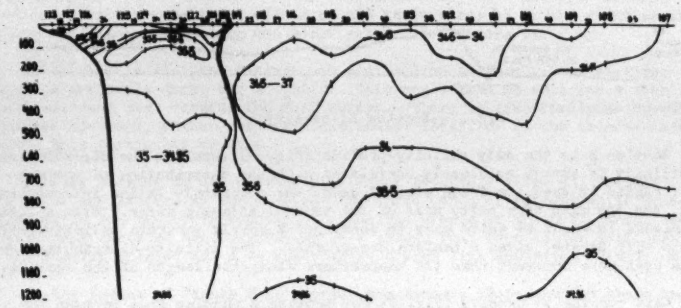

Fig. 6--Section B, salinity

When we come to the density-profile the current is split into three branches, the northern one being the strongest, and there is an indication of a weak counter-current between the branches.

The profile of Figure 5 at first seems to be much confused by the wavy nature of the isotherms. It must be remembered that this is an 850-mile section with the stations spaced on the average of every 45 miles. The other sections shown have had stations every 15 miles. Also this is now the center of the Atlantic and storms, tidal waves, and differences in barometric pressure have a better chance to cause displacements in the layers of the deeper waters. There has been much written about the probable occurrence of these submarine waves in the ocean and they have been called boundary-waves because they arise between two honogeneous layers of water which have a slightly different density. Along the Atlantic coast the circulation is intense enough so there is less chance of the under water-layers being disturbed than there is in this profile. An interesting speculation is brought up by the mass of water appearing at the northern end of the profile. The temperature and salinities of all but the superficial stratum correspond closely with the distribution of these characters in the intermediate band off our Atlantic coast. One is tempted to say that the intermediate band is continuous with the homogeneous mass of north Atlantic water, the edge of which just appears in this section.

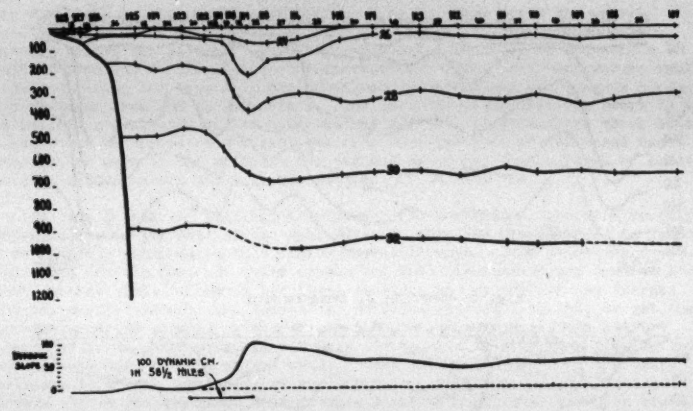

Fig. 7--Section B, density

Section B is the only salinity-profile (Fig. 6) shown as the distribution of salinity is always so closely correlated with the distribution of temperature. Again is seen the fresh coastal band, the moderately saline intermediate band, and the much more salty mass of the central Atlantic water. With a little experience it would be quite easy to construct a fairly accurate salinity-profile of the Gulf Stream, given a temperature-profile. The salinity-distribution remains even more constant than the temperature along the length of the current.

Next turning to density (Fig. 7) for Section B running from Bermuda to Delaware Bay, it is noted that the density-lines are comparatively horizontal except at the outer edge of the intermediate band where they all take a drop at the exact spot where the temperature and salinity-wall was located. It is desirable to explain more fully the meaning of these density-lines. Each line passes through water of the same density. In this case then the water inside the Gulf Stream is heavier than the water at the same depth outside the Gulf Stream. This can be seen by following offshore at any given depth. For example, follow out along the 200-meter level. In the intermediate band the water at this depth has a density of about 1.028. But across the temperature-wall on the other side of the Gulf Stream the water has a density of much more nearly 1.026. Now the tendency of the lighter water, by the principal of Archimedes, is to flow out over the heavier water, that is, in a westerly direction. Therefore, in this band of slanting density-lines at each depth there is a force present which tends to move the lighter water towards the west. This force is strongest near the surface and gets less as the depth increases. The extent of the force is accurately indicated by the slope of any given density-line. At a depth at which there is no such force the density-lines would flatten out. This is called the base-plane and in this profile lies just deeper than our observations extend. Without the rotational effect of the Earth then, this profile would indicate a westerly shift of the lighter water, but just as soon as movement takes place the rotational effect of the Earth acts and turns the current to the right. The movement then represented by these density-lines is towards the north; they show a current flowing through the profile and away

from us.

The lower portion of Figure 7 shows the dynamic slope across the section as found by the use of Bjerknes' formula; note that the slope amounts to very nearly 100 dynamic centimeters in a distance of 58 miles. Thus a fact discovered in the course of current-meter work in 1883 by Lieut. J. C. Fremont in command of the schooner Drift is clearly demonstrated, namely, that it is not the warmest water which is flowing north as the Gulf Stream, but it is the water just inside, that is, the temperature-wall itself, which constitutes the mighty current.

The studying of currents by means of calculating the dynamic slope across each profile is just the newer method of showing the same condition that this density-profile shows. The dynamic slope plotted below shows only the total result of the distribution of temperature and salinity as it affects the surface. The water on the right was warmer, therefore expanded. It can be thought of as having a higher surface. Water runs down hill, so a westerly current is initiated, but the rotational effect of the Earth turns it to the right so that the current flows north along the slope rather than across it.

After station 119, the warmest and most saline station in the picture, the slope gradually drops off slightly. This would seem to call for a weak counter-current just outside the Gulf Stream. There is practically no dynamic cause for movement, either over the continental shelf, or in the intermediate band in this particular profile.

Finally turning to the density-profile of Section J (Fig. 8), running north from the Azores, we can make out the three bands of current which were suggested in the temperature-profile. Thus one branch of the Gulf Stream seems to pass just north of the islands, while a second runs east in the latitudes of the English Channel and a stronger third branch lies 150 miles further north.

At the bottom of Figure 8 is plotted the dynamic slope and the three bands of easterly current are indicated. The marked hump seen towards the northern end of the profile calls for a comparatively strong narrow band of counter-

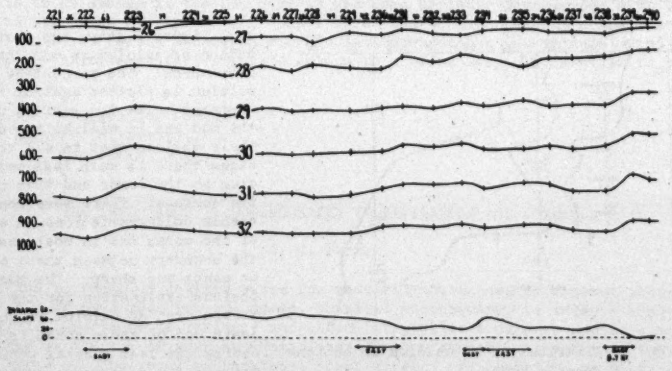

Fig. 8--Section J, density

current. Just under this hump is a region known as Faraday Hills, a small spot shoaling to 600 fathoms. It might be that the hump represents a clockwise eddy around the shallow spot. A clockwise circulation around shoals has frequently been observed in shallow water.

This profile is chiefly interesting because of one most unexpected feature, namely, the deeper density-lines do not flatten out. The driving force of the currents called for by the slanting density-lines comes mainly from below 500 meters. Now this profile crosses the North Atlantic Drift, which long ago was explained as a current resulting from the prevailing westerly winds. Velocity-calculations show that at places on this profile there are sufficient dynamic forces to produce a surface-current of 0.7 knot. Here is a case where an oceanographic phenomenon was written up in the text-books as a wind-current and nobody questioned the fact. Our profiles show that the circulation of the north Atlantic is by no means yet understood and there is much more research needed to fully understand the first principles of physical oceanography, namely, the circulation of the North Atlantic.

As indicated, our profiles bring up all sorts of questions concerning the causes and forces which bring about the distribution of the water-masses which constitute the underlying causes of the Gulf Stream and, for that matter, all truly oceanic currents. We are not yet ready to discuss these very interesting but difficult questions. Instead a few words may be said about another phase of our work, namely, our beginning at the study of the stratification of the deep-sea oozes--a fascinating field and one which promises great possibilities. Last summer we undertook to bring up cores of the mud from deep oceanic water. A heavy sounding-tube with a valve at the lower end was designed and various devices installed on the schooner to enable us to successfully handle the 600-pound sounding-tube and the two tons of wire cable. This was made only a minor part of our summer's program because we were not confident enough that sufficiently long cores of the bottom-sediments could be secured. The sounding-tube was lowered eight times and it came up empty only once. Twice we struck hard volcanic bottom off the Azores and were rewarded with but a handful of igneous rock. Two of the mud-cores were only about a food long but, finally having learned how to handle the gear, the last three attempts produced three-foot cores with the mud-layers undisturbed by their enforced entrance into the sounding-tube.

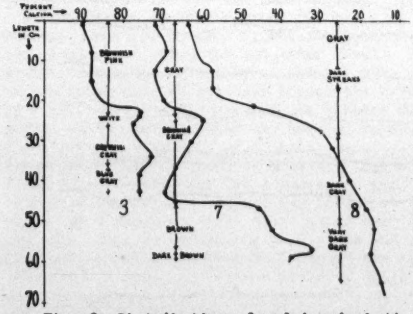

Fig. 9--Distribution of calcium in bottom-sample cores

Figure 9 shows the distribution of calcium in our three best cores. The percentage of calcium is plotted against the distance below the surface of the mud and it will be immediately noticed that in all three cases there is much less calcium at the lower end than near the surface. There were considerable color-variations in each of the cores and in most cases the boundary between these color bands was sharp. The most obvious explanation for the marked drop of calcium at the lower end of these cores is of course the last glacial period. Present-day cold-water deposits

show a much lower percentage of calcium than warm-water deposits. To go more into details consider core 8: The upper 25-cm section is globigerina ooze and contains practically no minerals except a few small pebbles obviously dropped by ice from time to time. In this section of the core there can be found about 14 species of forams. The lower 25-cm section is blue mud containing up to 10 per cent mineral matter. There are again a few scattered pebbles, but the bulk of the mineral matter is made up of quartz-grains up to one mm in diameter. No quartz was found in the upper part of the core. The blue mud contained only four species of forams. This core came from 130 miles off the coast of Ireland and was brought up from 1,800 fathoms. The upper 25 cm then represents the accumulation since the glacial period. The next 15 cm represents the accumulation during the time that the climate was gradually warming up and the last 25 cm was laid down under arctic conditions, vigorous currents bringing out the quartz-grains from the then nearer shore-line.

Core 7 taken from 400 miles due west of Ireland confuses the above reasoning slightly for the lower end of it contains just as many and just as large quartz-grains. Were they brought out and dropped by ice, or were they carried out by currents?

It will be noticed also that there is a sharp drop in calcium-content at about the same depth in all three cores, namely, at about 19 cm. This upper layer must have been laid down during a climate similar to the present. Below 19 cm the climate was gradually warming up after the cold of glacial time. Since the upper parts of all three cores consist of globigerina ooze and came from places that today have quite similar surface-temperature conditions, it is reasonable to suppose that they were all laid down at about the same rate. Although the material is very scanty, these cores seem to give a much slower rate of deposition than is generally supposed. Perhaps one cm per one thousand years is of the right order of magnitude.

One other theory that would account for lower percentages of calcium towards the bottom of deep-sea mud-cores is that there is a gradual dissolving of the lime going on under the surface of the mud. But the examination of our samples shows that the foram-shells 60 cm below the surface of the mud are in just as good a state of preservation as those near the surface. Everything points to no change in the mud once it has been buried. Next summer with improved apparatus we hope to get longer cores and perhaps penetrate the most recent glacial period and reach a layer laid down during interglacial times, where the calcium-content ought to return to a value very near what it is today.

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THE IMPORTANCE OF OCEANOGRAPHY TO NAVIGATION AND COMMERCE

W. E. Parker

Due to recently devised means for measuring ocean-depths without stopping the ship or even reducing its speed, physical oceanography is today a subject of great importance to mariners and undoubtedly will be of more and more concern to them as the use of such apparatus becomes more general. I refer to the various types of echo-sounding machines, which already are installed on many naval vessels, both American and foreign, and on a considerable number of commercial

ships. One has but to read the enthusiastic reports of those ship-masters who have used such apparatus in guiding their ships into port to be convinced that this new aid to navigation will "take" with merchant-seamen, as it has with hydrographic surveyors and others, and that within the near future all "well-found" ships will be provided with such apparatus.

The shallower parts of the oceans have always received the attention of hydrographers since at those places there may be shoals or reefs that must be avoided by ships or there may be valuable fishing-banks there. But in the open, deep parts of the oceans, hydrographers have taken but little interest, regarding information as to the exact depths there as of no concern to mariners or fishermen, those for whose benefit hydrographic establishments are maintained. The one exception to this condition has been the demand for examination of routes suitable for submarine-cables, which has resulted in a few lines of soundings extending from continent to continent or from island to island and throughout all depths of the intervening water. Otherwise, exploration of ocean-depths has been left almost entirely to the scientist.

Until the advent of echo-sounding, the measurement of great depths was a slow, tedious operation that could be performed only under favorable condition of wind and sea, since it was necessary to hold the surveying ship in exactly the same place for an hour or two, depending upon the depth, while the sounding-weight was slowly lowered to the bottom and the wire recovered on completion of the sounding. The cost of the work has been so considerable that only occasionally have governments or individuals been willing to provide the necessary funds solely for the advancement of science. Consequently, little is known today regarding the depths of oceans beyond the continental shelves, and of vast areas, nothing whatever is known.

Heretofore, the mariner's interest in depths ceased when he left "soundings," that is, depths of about 100 fathoms, and he had no further interest in that subject until he arrived again "on soundings" when approaching the port of destination. He could, if necessary, take a sounding up to 100 fathoms, but not much beyond that depth. Today a ship equipped for echo-sounding is always "on soundings." The master may, if he chooses to do so, take soundings continuously, day and night, from port of departure to port of destination without interfering at all with the progress of his ship and with very little effort on his part or on the part of his watch-officers. Now, if his chart gives the depths out into deep water as it does along the continental shelf, he will have a means for checking his position and correcting his course while still far from port. The ocean-bottom is not flat and featureless, but, on the contrary, abounds in hills and valleys so that a mariner crossing one of these charted features can, by soundings alone, fix the position of his ship with considerable accuracy, probably with at least the accuracy possible from sun-sights, and can do that irrespective of the weather.

Echo-sounding depends upon the fact that the sea-bottom gives back an echo from a sound produced under water and also upon the fact that sound travels through water at a constant velocity, varying only with changes in the temperature and density of the water. And since the mean effective temperature and the density are rather constant in any particular locality and may be estimated with considerable accuracy, the correct velocity of sound at any place can be readily obtained with sufficient accuracy for navigation.

There are several different types of echo-sounding apparatus in this country and abroad, but all are alike in this respect: They have a means for producing a sound-wave or a train of sound-waves under water, usually near the

keel of the vessel; a means for receiving and reporting the reception of the echo from that sound; and a means for measuring the time-interval between the emission of the sound and the reception of the echo. With one exception, the receiving units of all types of echo-sounding apparatus are similar in principle, although quite different in form. The common type of receiver is the hydrophone or carbon-button of the telephone-transmitter enclosed in a rubber or metallic casing. The sound-producer and the timing-device vary greatly among different types of sounding-apparatus.

The first echo-sounding machine that got beyond the experimental stage in this country was produced by the United States Navy. It was christened sonic depth-finder. In this type the sound was produced by an electric oscillator set into the hull of the ship near the keel. Reception of the echo was by means of a battery of hydrophones of the rubber-incased type installed either in a water-tank within the hull of the vessel or within a shield, known as a "blister," secured to the outside of the hull-plating. Timing was by means of an ingenious device driven by an electric motor which was maintained at constant speed by means of a tuning-fork governor. The oscillator was put into operation by the closing of an electric circuit through the timing-device, thus causing the oscillator to vibrate for a brief interval whenever the electric circuit was made. Short series of sound-waves traveled from the oscillator to the bottom and returned to the vessel where they were picked up by the hydrophones and reproduced as electric waves in one side of a head-phone receiver of the radio type. The outgoing sound-waves, or rather corresponding electric waves, were led to the other side of the head-phone. The interval between the series of sound-waves could be varied slightly by shifting contacts in the timing-device so that a train of outgoing sound-waves could be made to synchronize with a train of returning waves from some previous sound. The timing-device enabled one to determine the exact time-interval between groups of sound-waves and how many groups were in transit between the oscillator and the hydrophones, thus affording a means of measuring the time required for sound to travel from the bottom of the ship to the sea-bottom and return. One-half this interval multiplied by the mean velocity of sound along sound's path gave the depth.

The sonic depth-finder gave very accurate measurement of all depths from about 100 fathoms to the greatest of ocean-depths. Below 100 fathoms its accuracy decreased rapidly so that at about 80 fathoms it was no longer reliable. This apparatus was also equipped to measure the angle of the path of the echo with the horizontal. Since the oscillator, the hydrophones, and the spot on the sea-bottom from which the echo came form an isosceles triangle of which the base and adjacent angles are known, the hypotenuse, in this case the depth, could be computed. This method was intended for use in depths too shallow for accurate measurement by the time-method, but experience showed that a high order of accuracy could not be attained with the sonic depth-finder used in this way.

In the year 1924 the Submarine Signal Corporation of Boston, Massachusetts, brought out an echo-sounding machine to which they gave the name of Fathometer. This machine was intended for use on commercial vessels and was designed primarily for use in depths under 100 fathoms. Accordingly, emphasis was placed on ruggedness and simplicity of operation for shallow depths. However, a second method of operation was provided also so that greater depths could be measured if desired. Depths as great as 2,500 fathoms have been measured by means of this machine.

Sound is produced by an electric oscillator similar to that used with the

sonic depth-finder but smaller and requiring but a tenth of the electrical power. The echo is received by means of a single hydrophone placed in a water-tank which is bolted against the inside of the shell-plating. The timing-device closes the circuit which actuates the oscillator and receives and records the echo, or rather an electric impulse caused by the action of the echo on the hydrophone. For depths to 100 fathoms (depths in excess of 200 fathoms have been measured by this method) the machine measures and records depths entirely automatically. A disc carrying a neon-gas tube revolves at uniform, known speed back of a scale of fathoms painted on a glass plate. Reception of an echo sends a high-voltage current through the neon gas causing it to give out a flash of red light which shows against the fathom-scale. Sound impulses are sent out at the rate of four per second and the echoes being recorded at that rate cause the neon tube to seem to glow with but a slight flicker, since the eye carries over from flash to flash retaining the impression of a nearly steady illumination. Depth-measurement to the limiting depth at which an echo is capable of actuating the neon tube involves merely reading the scale at the place where the red light appears to stand; an operation no more difficult than noting time on a clock.

When the echo becomes so faint, because of the distance that sound and echo have traveled, that it will no longer actuate the neon tube, it can still be heard in head-phones placed in circuit with the amplified current from the hydrophone and if heard at all can be readily recognized by its peculiar musical note. The echo from the usual type of fathometer oscillator can be heard from a distance of 2,500 fathoms, and apparently any depth would be attainable with this machine given a powerful enough oscillator.

To measure depths beyond the range of the neon tube, a gear is shifted in the timing-device so as to reduce the speed of the revolving disc and to light a small incandescent lamp attached to the disc. This lamp travels in contact with a second scale and appears as a band of white light passing along that scale. Head-phones are placed in the amplified hydrophone-circuit and the observer notes the position of the white band with reference to the scale when he hears the echo in the head-phones. Some practice is necessary before accurate results can be obtained by this method and apparently some persons never do get very good results. Average observers will, after some practice, get results agreeing within five to fifteen fathoms. However, the observed depth may differ from the actual depth by a much greater amount since observations are necessarily made in terms of some arbitrarily fixed velocity of sound to which the speed of the timing-device has been adjusted. Usually a mean velocity is adopted, the timing-device adjusted to that velocity, and the observed depths corrected later for the difference between the actual mean velocity of sound along the path of the sounding as determined from observations on temperatures and salinity and by means of tables and the velocity to which the machine had been adjusted. This latter operation, however, is more for the surveyor than for the mariner who does not need the precision sought by the former.

Within the last year another type of fathometer was brought out by the same company. This machine has the red-light method only, has an electrically operated hammer in place of the oscillator, and is smaller, simpler, and more compact than the one described above. Measurements of depths as great as 500 fathoms have been reported from one installation in which an exceptionally powerful hammer was used. This hammer, by the way, operates in direct contact with the ship's plating, striking a sharp blow on the inside of the plating whenever the hammer-circuit is broken by the timing-device. Experiments with one such machine by officers of the Coast and Geodetic Survey showed that soundings can be taken with very great accuracy and that, in general, there is

less likelihood of an erroneous sounding being taken with this machine than by lead-line or by any other method of sounding.

Experiments in echo-sounding have by no means been confined to this country. In England and on the Continent, especially in France and Germany, much attention has been given to this subject. In each of these countries at least one type of machine has been devised and has been used rather extensively. These machines differ among themselves and all are different from the American types.

In Great Britain the lead in echo-sounding has been taken by the Admiralty which has brought out two machines, one for use in deep water and one for shallow water. Both are similar in principle but somewhat different in design. Sound is produced by impact-transmitters, which in the shallow-water apparatus is by means of a blow from a spring-drive hammer and in the deep-water apparatus by means of a pneumatic hammer, both controlled electrically from the timing-device. Reception of the echo is by means of a hydrophone, not unlike the American type. The timing-device has a drum driven by a constant-speed electric motor and this drum is in circuit with the telephone-receivers of the hydrophone-circuit except when one of the contact-brushes passes over an insulated segment of the drum. The echo can, therefore, be heard only when the telephone-brush is on the insulated segment. The brush can be moved in the path of the revolving drum and, therefore, the position of the brush indicates the amount the drum has changed position since the sound was produced, as the transmitter-circuit, which controls the release of the hammer, is controlled by a second drum operating in unison with the timing-drum. The amount the telephone-brush is displaced is indicated on a depth-scale, from which the depth of water is read when the echo is heard in the telephone-receiver.

In France the ultra-sonic sounding system as proposed by Langevin appears to have met with favor, but the French have also an acoustic sounding-apparatus which was devised by Marti of their Hydrographic Service. In the former type of sounder, no separate receiving-apparatus is required as the quartz-projector, which emits the ultra-sonic waves, also receives the echo from these emissions. The time-measuring device appears to be rather complicated, but great accuracy is claimed for it. It is also claimed these waves can be directed in a beam, or at least in a narrow cone, so that the echo comes from a spot or small area on the sea-bottom which can be definitely located with respect to the survey-ship.

The Marti system has three methods of producing sound under water, depending upon the depth to be measured. For very shallow water a hammer, operated by a coiled spring, is used. This hammer strikes on some part of the ship's frames near the keel, whence the sound is communicated to the water through the ship's plating. For greater depths (up to 1,500 meters) sound is produced by the impact of a rifle-bullet on the surface of the sea. And for the greatest depths a charge of explosive material is detonated in the water astern of the ship. Reception of the echo is by means of a hydrophone submerged in a water-tank within the vessel. Time-intervals are measured on an oscillograph. The outstanding German echo-sounder is the Echolat invented by Dr. Alexander Behm of Kiel. In this apparatus sound is produced by the explosion of a cartridge under water from a fitting in the vessel's hull. Reception of the echo is by means of hydrophones, as in nearly all other types. Depth-recording is visual and by means of a band of light on a graduated scale of depths.

It appears, therefore, from the wide-spread interest that has been manifested in this subject and from the great progress that has been made to date

that we may reasonably expect within a very few years an echo-sounding machine simpler and more dependable than any yet devised and obtainable at a much lower cost. Meanwhile, the importance of this appliance will have become more fully appreciated among seamen and ship-owners and then it will be regarded as one of the essential articles of equipment for all "well-found" ships.

When that day comes, and I believe that it is not far off, mariners will not be satisfied with existing charts. They will demand that the continental shelf be accurately surveyed to its outer limit and that surveys be extended further, even out into the ocean-depths. They will want all of the principal hills and valleys that lie submerged in the oceans, the pronounced and characteristic changes in depth, accurately shown on their sailing-charts so that they may use them as "land marks" in shaping their course in thick weather and at night.

Such surveys are outside the usual field of activity of the hydrographer, as accepted heretofore, and are more nearly within the recognized province of the physical oceanographer. However, the soundings that will reveal the configuration of the ocean-bottom, to be of full value to the mariner, must be fixed geographically with all possible accuracy--with much greater accuracy than has been considered sufficient in such surveys as have sometimes been made for scientific investigation, such as for the construction of a bathometric chart. Consequently, the methods employed by the hydrographer and the standards of accuracy recognized by him should be applied in such ocean-surveys.

U. S. Coast and Geodetic Survey,
Washington, D. C.

SECTION OF OCEANOGRAPHY

(Washington, D. C., second meeting held jointly with the general meeting of the Union, April 26, 1929)

THE GRAVITY-MEASURING CRUISE OF THE S-21

C. S. Freeman

(Abstract)

In a recent book-review, the writer-critic spoke of science today as developing at an unprecedented and somewhat embarrassing rate, and indicated that, in consequence, there exists in current literature and thought a tendency to neglect the historical or humanistic aspect of science.

For the laymen in the world of physical science, here lies apparently a field for our endeavors, a constructive outlet for the interest some of us have in scientific progress--an interest no less keen than the maturer and better-informed enthusiasm which invests those who are devoting their entire lives and undivided thought to the many problems in specific lines which still remain unsolved.

The U.S.S. S-21 is one of our later types of submarines, although not the latest type, with surface-displacement of about 850 tons and submerged-displacement of an additional 200 tons. Her overall length is 219 feet and her beam 21 feet. During the gravity-measuring cruise, she was commanded and still is commanded by Lieutenant J. L. Fisher, U.S.Navy, and was manned by three additional officers and 39 men. The scientific party consisted of Dr. F. A. Vening Meinesz of the Geodetic Commission of Holland, Dr. Fred E. Wright of the Carnegie Geophysical Laboratory of this city, and Elmer B. Collins of the Hydrographic Office of the Navy. To provide a surface-escort for the submarine, the Navy Department detailed the two Eagle boats customarily employed as a unit for experimental research work along strictly naval lines. Lieutenant-Commander T. L. Nash, U.S.Navy, who commanded the Eagle Division, was, by virtue of his rank, in command of the entire expedition.

In the 56-day cruise, October 2 to November 27, 1928, from Washington to Washington, the submarine cruised approximately 7,000 miles and established 46 gravity-stations at sea, besides taking gravity-measurements in the harbors of Hampton Roads, Virginia; Key West, Florida; Galveston, Texas; Guantanamo Bay, Cuba; San Juan, Porto Rico; and St. Thomas, Virgin Islands.

The pendulum-apparatus of Dr. Vening Meinesz, in order to counteract the unsteady platform-effect of a vessel at sea operating on a swinging pendulum, employs three pendulums, A, C, and B, permitting the middle pendulum, C, to remain stationary, while giving small equal and opposite swings to pendulums A and B. The small horizontal acceleration caused by the uncontrolled movement of the ship, of course, affects the three pendulums equally, but by considering C paired with A and recording their movement as a pendulum-pair and similarly considering C paired with B and recording their movement as a pendulum-pair, the record of each pair so registers as to indicate only the differences in the angular changes between the two pendulums and hence the record is freed automatically from the effects of horizontal accelerations. Each pendulum-pair acts like a simple pendulum unaffected by the horizontal disturbances that take place within the limits of tolerance of the apparatus. The gimbals of the apparatus used aboard the S-21 permitted only a roll of about 7° on a side.

The record of the movements of the pendulum-pairs is made, through the agency of a mirror-system, on a strip of moving photographic paper, which registers simultaneously: The time-signals from two chronometers, set to sidereal and mean time; the variations of temperature of the air-space in the apparatus; the movements of two additional damped-pendulums, one swinging in a plane parallel to the plane of the pendulum-pairs to register ship's roll, and the other swinging in a plane at right-angles to the first to register ship's pitch; and finally the vertical accelerations due to wave-action which give rise to a rhythmic fluctuation of the time-signal breaks. The temperature and humidity of the air within the apparatus are taken account of, and also the temperature and barometric pressure outside.

The operation of making one record for a given station occupies about 30 minutes. The diving time from the moment of submergence to the leveling out and steadying down of the submarine varies, of course, but would scarcely average more than six minutes. Consequently the time under water for occupying one station rarely exceeds an hour, and perhaps averages close to 40 or 45 minutes. Latitude, longitude, course, speed, depth of water, and direction and velocity of current are all a part of the record of a station. The east or west component of speed due to ship and current is applied with proper sign to the Earth's rotational velocity in computing the resultant centrifugal acceleration. In short, in spite of the many variables entering into the computations, the greatest possible precision is aimed at and the gravity-result, it is believed, is accurate to one part in 300,000. The ultimate value as computed derives from the standard gravity-formula $g = 4\pi^2 l/T^2$, where l is the length of the pendulum, in this case a fictitious or constructive pendulum, and T is the time of vibration. The difference between the g-value for any particular station and the g-value obtained by isostatic reduction is the gravity-anomaly.

Perhaps there is no more obvious an example of the close relationship between a basic problem of astronomy, involving a series of observations responsive to that problem, and another problem not immediately associated in our minds with astronomy than is revealed in gravity-determination measurements. The unit of measure in astronomy has for centuries been the radius of the Earth's orbit derived from the Sun's mean equatorial horizontal parallax, but the actual mileage in this unit is determined only by an accurate determination of the diameter of the Earth. This in turn can only be determined from a knowledge of the figure of the Earth, that is, its form. The form of the Earth can be determined by geodetic measurements of distances on its surface. The earliest recorded measures of this sort were made by astronomers. Such real measuring has a double value, because it results in giving us knowledge of both form and dimensions. There is a distinct astronomical method of determining the form of the Earth by taking account of nutation and precession, as well as by noting some of the irregularities in the motion of the moon, of which there are far too many. The third method of determining the form of the Earth is the one with which we are here concerned, namely, by observations of the varying force of gravity at various points in various latitudes.

While determining the form or figure of the Earth in this manner, to serve astronomical ends, we are, in addition, directly contributing to our knowledge of isostasy.

It is by means of gravity-measurements that we determine to what extent the Earth is in a state of isostatic balance. We speak of the Earth as an oblate spheroid, although we know its surface to be a mass of irregularities--continents rising above the sea, mountains rising above plains, ocean-deeps descending far below ocean-basins. In like manner, we use the term isostasy

to convey the idea that the surficial shape of the Earth is an expression of the figure of equilibrium which would perforce be attained by a mass of material of the properties, dimensions, and rotational velocity of the Earth, which had spun sufficiently long to become static. It has been pretty thoroughly determined that isostatic balance is the rule over the great land areas of the world, but these areas form less than 30 per cent of the Earth's surface. It has been and continues to be the aim of the International Geodetic and Geophysical Union to extend gravity-determination work to the ocean-beds with a view to finding out whether isostatic balance is the rule there also, and to learn too whether, over those curious trough-like depressions or wrinkles known as ocean-deeps, there exist the large negative anomalies we might expect, as showing a deficiency of mass in the down-warped crust. Dr. Vening Meinesz has made it possible, with his pendulum-apparatus, to pursue these investigations and the cruise of the S-21 represents what we hope is only an initial contribution to this highly important work.

As a beginning, areas were chosen for investigation that gave promise of anomalous results. The cruise was one of reconnaissance, as was, in a somewhat different sense, the cruises of the Dutch submarines which placed a circuit of ocean gravity-stations around the world by way of Holland, the Suez and Panama canals, and the Dutch East Indies. It is conceivable that we may expect reconnaissance-cruises to be the rule for many years to come, because the areas to be covered are so vast as to invite an initial outline of the general gravity-characteristics of the various ocean-beds before beginning the detail exploration that will eventually be in order. Of course, it is possible that some detail work will go on simultaneously. For, in this connection, it seems pertinent to mention that, as a result of correspondence between Dr. Henry B. Bigelow, representing the Committee on Oceanography of the National Academy of Sciences, and representatives of the Navy Department, there has been approved by the Secretary of the Navy a series of recommendations of a special naval oceanographic board looking to an initiation of oceanographic investigations which, although planned on a modest scale, are yet designed eventually to cover extensive areas. In the absence of the Hydrographer of the Navy, I may say that the Hydrographic Office of the Navy has already prepared general charts showing the tremendous areas of ocean the depths of which have certainly never been scratched, however frequently the surface may have been traversed by the shipping of the world.

The commonly accepted theory of the constitution of the mass of material which makes up our Earth visualizes an inner dense core which is presumably fully compensated throughout and represents, doubtless with great accuracy, the ellipsoid of revolution that gives to the planet its general spheroidal form. Merged outwardly on this inner core is the crustal portion of the Earth that is supported, generally speaking, in a balanced condition by the inner dense core. This condition of equilibrium is departed from in a positive or negative way, depending on local conditions that are ordinarily subject to quite obvious explanations. These positive and negative anomalies of gravity may be so great as to serve as an indication that the Earth's crust in their vicinity is critically unstable. The difference between critical instability where earthquakes may be expected and an instability of an order that may be considered as not critical cannot be said to be known at this time. It is obvious that everything about this distinction should be known. Gravity-measurements, especially measurements over uncompensated areas, will provide a means of determining this distinction, this difference; of finding out, in other words, the forces or stresses that enter into the production of earthquakes and mountains and ocean-deeps and other crustal phenomena.

The cruise of the S-21 furnished data showing that the Mississippi Delta--

that is, the portion of it to seaward--is practically compensated. There was naturally some doubt of this fact, because several billion tons of sediment are borne into the Delta waters every year. Over the deeper parts of the Atlantic, the Gulf, and the Caribbean, exclusive of the so-called "deeps," the S-21 found rather large positive anomalies for which it is understood no entirely satisfactory explanation has yet been offered. The anomalies in connection with the Nares Deep, the east end of the Bartlett Deep, and a small unnamed deep between St. Croix and St. Thomas indicate recent uncompensated geologic features involving large shearing stresses.

The passage of Hatteras on the final run north was marked by the unusual combination of high wind, heavy sea, thick fog, and hard snow-squalls at intervals. This combination produced such rough underwater-conditions that it was just possible to secure a single gravity-observation at a depth of 85 feet and where two subsequent dives of 100 feet still discovered at that considerable depth a roll of more than 14° amplitude.

From an operating standpoint, the cruise was a success and those competent to evaluate it state the cruise was also scientifically successful. During the past 35 years the United States has occupied about 350 gravity-stations. Forty-nine of these were occupied by the S-21 in her two-month voyage. The expedition itself was an example of the possibilities of coordinated effort between Government and civil agencies, and in its actual carrying out affords a valuable instance of the possibilities of international cooperation in scientific research and investigation. The Carnegie Institution of Washington, the Coast and Geodetic Survey, and the Navy were all concerned in contributing to the success of the endeavor. In considering Dr. Vening Meinesz's invaluable personal contribution to this success, we must bear in mind that the Netherlands Geodetic Commission consented to his extending the benefit of his experience and technique and the American National Academy of Sciences made possible the meeting of certain expenses inseparable from such enterprises.

It is in the free masonry of science that lies a great part of its humanistic appeal. Activities which promote such fellowship and sympathy, which recognize that the harvest fields of knowledge are sufficiently wide to permit, even on the part of those whose ultimate aims are diverse, of common effort, free from selfishness and any thought of aggrandizement, are the kind of activities calculated to gain for science its greatest and swiftest rewards.

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OCEANOGRAPHY AND THE FISHERIES

Henry B. Bigelow

This paper was presented informally and no manuscript or abstract was prepared.

THE COOPERATIVE SURVEY OF LAKE ERIE

Charles J. Fish

(Abstract)

This report (published in full under the title "Preliminary report of the cooperative survey of Lake Erie--season of 1928, in Bull. Buffalo Soc. Nat. Sci., v. 14, No. 3, 7-220, 1929) gave a preliminary statement of the results of a three-month survey of eastern Lake Erie in 1928 to determine the cause or causes for the decline in the fish-supply. The work was carried on under the joint auspices of the United States Bureau of Fisheries, the New York State Conservation Department, the Ontario Department of Game and Fisheries, the Health Department of the City of Buffalo, and the Buffalo Society of Natural Sciences. These five organizations pooled their resources, personnel, and equipment to function as a single unit, thus avoiding duplication of effort and offering maximum opportunities for cooperative work.

The average yearly catch in Lake Erie alone during 1913-1925 was 58 million pounds; that of the other lakes combined 64 million pounds. But the supply gradually diminished, areas which once supported large quantities of fish becoming in some instances practically depleted. In the ten years (1915-1925) the catch declined one-half. In 1927 the worst slump in history was experienced and it became evident that something must be done at once if the industry was to be saved.

Legislative action was retarded by a diversity of opinions on the factors responsible for the decline. Of the many theories advanced the following received the most general support: (1) Overfishing; (2) destruction of aquatic life by chemical and sewage wastes from the cities; (3) invasion of spawning-grounds by sewage-silt, thus rendering large areas unfit for production; (4) lack of food; and (5) destruction of eggs and fry by storms.

The work done was reviewed under three heads: Physical hydrography; bacteriology and chemistry; and biology.

Lake Erie, the shallowest of the Great Lakes, is in form a shallow glaciated basin ranging over much of its area from ten to fourteen fathoms in depth. At the eastern end is a so-called "deep hole" of approximately 616 square miles (depth greater than 30 meters) at one point reaching a depth of 70 meters. In cross-section the Lake is on either side bounded by sandy beaches or limestone cliffs sloping gradually into coarse sand and finally beyond the influence of land outwash into the basic smooth shale, honeycombed shale, or faulted rock-bottom, covered with almost no sediment of any sort except for short distances off the mouths of the larger rivers.

Regarding the water-mass, it has been said that Lake Erie in storms is churned from top to bottom and in all probability great numbers of young fish and fish food destroyed. Such conditions may occur in the western area of the lake but were this true in the eastern part, one would expect to find in summer almost uniform temperatures from surface to bottom, but this was not the case, at least during the period of the present investigation. Descending from the surface the temperature was found to decline gradually until in late August a level beyond the influence of summer heating was encountered at about 20 meters. Over a considerable area this cold layer of bottom water of 4° to 5° C covered the floor of the basin. At times it oscillated back and forth. One week it was found to have advanced 30 miles toward Buffalo and by the next cruise it

retreated again even beyond its former position. The cause of this movement has not been fully determined as yet, although when compared with available meteorological data it would appear that the oscillation of the cold bottom-layer to the east in each case occurred as the water, piled up at the eastern end of the lake by westerly winds, was readjusting itself to the normal lake-level. The rise in the level in the eastern part of the lake was accompanied by a retreat of the bottom-layer. Thus a close correlation occurred between the wind and the movement of the water-mass, the advance of the cold mass taking place in the readjustment process after the termination of the wind. This explanation, however, is merely tentative, and further observations will be necessary to substantiate or refute its validity.

Submarine currents were found, not always corresponding to those at the surface. Following storms or barometric irregularities the lake-level in readjusting itself forms the probable source of the strong subsurface-currents often reported in Lake Erie. The bottom-water in summer remains intact except for oscillations, the outpouring from the Lake into the Niagara River passing over a sill five meters in depth draws only from the uppermost levels. Provided that there was no internal friction the normal flow of water across the section from Long Point to the American shore could not exceed 0.1128 foot per second or 1.84 miles per day. This is the maximum current which could possibly result from the passage of water out of the eastern end of the lake, and as a matter of fact, this rate is considerably reduced by such factors as the viscosity, increase in width and depth of the area, etc. The natural flow of water as a current-forming factor destructive to nets or larval fish can, therefore, be discounted.

Not enough is known of seiche action to permit a statement on the maximum rate of subsurface movement possible in the water-mass. The maximum current velocity recorded was 0.92 foot per second or 0.63 mile per hour. No doubt more powerful submarine currents do occur at times, but however violent in force there is not the churning action found at the surface and, although no doubt an important factor in transporting eggs and young fry from one place to another, they probably do not exert serious destructive action.

The results of more than 500 chemical and bacterial analyses made from samples taken at the surface, mid-depth, and bottom throughout the area indicate that the lake as a whole is remarkably free from pollution. Lake Erie may be classified as a rather hard-water lake with a fixed CO_2-content of about 22 cc per liter computed from the carbonate and bicarbonate of calcium. The abundance of oxygen at the bottom throughout the summer (averaging 73 per cent in August and 72.4 per cent in September) in spite of the rich zooplankton fauna and the lack of vertical mixing indicates that stagnation in the lower levels does not normally occur in Lake Erie; it is highly oligotrophic.

Starting with the production of food-materials, Lake Erie's rich zoo- and phytoplankton population has apparently not declined with the fishery and compares very well in abundance with the most fertile ocean-areas. A striking characteristic of both the microplankton and the macroplankton was the inequality in horizontal distribution quantitatively and qualitatively. Production of plants and animals increased during the summer, the highest zooplankton production centering in the deeper part of the lake and expanding laterally until almost the entire offshore area was included within the 300 cc curve (300 cc taken in a five-minute meter net-haul). The height of production of zooplankton was found on the third cruise in the middle of August when the surface-temperature was highest. Later it declined in volume as the microplankton increased, the latter becoming increasingly abundant with the approach

of the autumnal maximum at the end of the season.

The general results of the survey indicate that Lake Erie is capable of supporting a much larger fauna of open-lake fishes than exist there at the present time. There are no dangerous silt-deposits affecting spawning-beds, food is present in abundance, and there appears to be no dangerous pollution of the offshore waters. Only those species which enter stream-mouths to spawn appear to be in danger of depletion from chemical or sewage pollution. The decline seems to have resulted from over-fishing and unwise fishing, and if this be true the remedy lies in the hands of those legislative bodies having jurisdiction over the lakes.

Buffalo Museum of Science,
Buffalo, New York.

WORK OF THE CARNEGIE TO DATE

William J. Peters

(Abstract)

This paper (published essentially as given in Sci. Mon.,v. 29, 97-108, 1929) gave a brief account of the program and route of the Carnegie during the first year of the seventh cruise which began May 1, 1929. The principal object of the magnetic work has been restricted principally to the determination of secular variation. The atmospheric-electric observations are even more extensive than on earlier cruises since this work had been inaugurated very much later than the magnetic.

The paper goes more into detail in describing observations and records of other ocean studies, physical, chemical, and biological. The undertaking of the extensive program has been made possible through the favorable consideration of the President, Dr. John C. Merriam, and the Trustees of the Carnegie Institution of Washington. Credit for the successful execution thus far of the program, first in the North Atlantic Ocean (May to October 1928) and subsequently in the Pacific, is due to the untiring interest and effort of the scientific staff of eight supported by the sailing personnel of 17. When the paper was prepared, practically one-quarter of the 110,000 nautical miles of cruising planned had been completed. (See also several papers in this Volume presented before the meetings of sections at the eleventh annual meetings.)

Department of Terrestrial Magnetism,
Carnegie Institution of Washington,
Washington, D. C.

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SECTION OF VOLCANOLOGY

(Washington, D. C., April 25, 1929)

VOLCANIC OCEANIC ISLANDS

Henry S. Washington

(Abstract)

Volcanic oceanic islands are of special interest because they furnish direct evidence as to the material that forms the ocean-floors.

The Pacific volcanic islands are arranged in linear groups, with a common general trend of about west-northwest and east-southeast. There are many active volcanoes in the Pacific. The Intra-Pacific lavas are mostly basalts, but with small amounts of trachytes and other alkaline lavas at almost all the groups. There are many coral islands in the Pacific, which are probably built on sinking volcano-pedestals. The Pacific is surrounded by folded mountain ranges and island festoons, on which are many volcanoes, some of them active. These Circum-Pacific lavas are chiefly andesitic and very different from those of the Intra-Pacific islands.

The Atlantic is more complex than the Pacific, with three principal volcanic regions. A narrow, submarine ridge extends down the center from north to south, about midway between the east and west coasts. On this is a line of volcanic islands, the Azores having the only active volcanoes. The lavas of these ridge-volcanoes are mostly basalts with small amounts of trachytes, as in the Pacific. Three main groups of volcanic islands lie on the western continental shelf of Africa, with a few active volcanoes. Basalts here are most abundant, but there is a somewhat large proportion of alkaline lavas. The rocks of the main Atlantic basin are decidedly more alkaline than those of the Pacific. In the north of the Atlantic basin are some volcanic islands, remnants of a basaltic plateau that formerly occupied this part of the basin. These arctic lavas are all basalts. Iceland shows the only certainly active volcanoes. There are very few coral islands in the Atlantic.

The Indian Ocean contains few volcanic islands and few active volcanoes. The Indian Ocean lavas are like those of the other two oceans.

The volcanic and other features of the various oceans suggest the idea that the Pacific is the youngest ocean, and that the Atlantic is older. This would apparently favor the hypothesis of the origin of the moon as an offshoot of the Earth from where is now the Pacific basin.

Geophysical Laboratory,
Carnegie Institution of Washington,
Washington, D. C.

THE VOLCANOES OF JAVA AND BALI

E. G. Zies

(Abstract)

In this paper a report is given of volcanological studies made in the islands of Java and Bali during the summer of 1928.

This extensive volcanic area is an especially favorable region for such studies because the general geological and petrographic features of the islands have been determined and because excellent field work is being carried out by the volcanological survey (Vulkaandienst) located at Bandoeng, Java. The writer wishes to express his great appreciation for the hearty cooperation on the part of Dr. Ch. Stehn, the director of the Vulkaandienst, and all of his associates.

In 1928 the activity throughout the islands of Java and Bali was in the solfataric or in the fumarolic stage. The writer visited a series of volcanoes in Western, Middle, and East Java. Samples of lava, fumarolic incrustations, and gases were collected for laboratory study. The percentage of the so-called insoluble gases in the gaseous emanations was determined in the field. It was found that contrary to our experience in the Valley of Ten Thousand Smokes the percentage of these gases was different at each volcano and seemed to bear a definite relation to the temperature of the emanations; the higher the temperature the greater the percentage of insoluble gases. It is quite probable that the ratio of these gases to the steam could be used in determining if the activity of any given volcanic vent is waxing or waning.

The alteration of the walls of the craters by the gaseous emanations is particularly noteworthy. At Tangkoeban Prahoe in West Java the andesite rock of one of the crater walls has been altered; alunite and sulfur have been deposited in the vesicles of the rock. At the sulfur volcano, Papandajan, alteration of the wall-rock has taken place on a huge scale. At the present time the rock of what is probably a lava plug (Kawah Mas = The Golden Crater) is being broken down by the sulfur-steam emanations (temperature about 300° C). Tellurium and arsenic sulfide are also present in the emanations and have been concentrated at the cooler periphery of each vent. Laboratory study will no doubt reveal the presence of other products that have been concentrated by virtue of difference in vapor pressure for a given temperature. This work is now in progress.

The volcano Merapi in Middle Java was emitting steam and hydrochloric acid from the lava plug that has been slowly extruded in the course of the past 40 years. The temperature of the emanations was as high as 500° C and to judge from the amount of alteration of the andesitic rocks of the plug the concentration of acid must be relatively high.

At Kawah Idjen, one of the rim volcanoes of the huge Idjen Caldera in East Java, the emanations contain both sulfur and hydrochloric acid. Here again the acid gases have altered the rock of the crater walls. The crater lake of Kawah Idjen is of considerable interest on account of the relatively high concentration of HCl and H_2SO_4 in the water (3.5 per cent of mixed acids). A detailed analysis of this water will be undertaken and especial stress will be laid on the presence of unusual elements and constituents.

At Batoer on Bali a basaltic cone is located within a great andesitic caldera. In 1926 a lava-flow broke through the wall of one of the craters of Batoer and deposited about 21 million cubic meters of lava in the caldera. During this period the fumarolic activity in the crater itself was insignificant but in the course of two years following the eruption the fumarolic activity increased considerably as is shown by the fact that, in 1929, the temperature of the emanations was in the neighborhood of 460°. The insoluble gases were present to the extent of about 50 per cent. Fumarolic activity here too was altering the rock of the crater and the idea suggested itself that such activity may, in an active volcanic area, play an important rôle in weakening the

wall rock and thus permit an imprisoned lava to escape.

Attention was also directed to the steam explosions that occasionally take place on the island of Java not in the volcanoes themselves but in cultivated fields where there was no indication of thermal activity previous to the explosion. The probable bearing on the formation of ore deposits through the medium of the imprisoned H_2O and relatively high concentration of H_2S and CO_2 was also discussed.

The results of both the field and laboratory studies will be presented in subsequent papers.

Geophysical Laboratory,
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Washington, D. C.

THE VOLCANIC HISTORY OF THE SAN JUAN MOUNTAINS, COLORADO[1)]

Esper S. Larsen

The San Juan Mountains, which are made up chiefly of volcanic rocks, are located in southwestern Colorado and occupy a rudely circular area of over 10,000 square miles, or larger than the state of Massachusetts. They have been studied and mapped geologically by the United States Geological Survey, chiefly by field parties under Whitman Cross and Esper S. Larsen.

There was no volcanism in the area from the beginning of the Paleozoic to nearly the end of the Mesozoic. During the late Cretaceous or early Eocene, a considerable mass of andesitic rocks was erupted and almost completely destroyed by erosion. Following a period of diastrophism eruptions again took place in Eocene time and continued with intervals of quiet into Quaternary time.

The first of these eruptions formed a cone about 15 miles across, chiefly of andesitic rocks but containing rocks with a great variety of textures and ranging in composition from quartz latite to basalt. The San Juan tuff was next deposited in the western part of the area. It is composed entirely of clastic, andesitic material, water-laid and fairly well bedded. It occupies an area over 50 miles across and has a thickness of over 1,000 feet. The exact source of the material is not known.

After considerable erosion the volcanic pile of the Silverton volcanic series (Oligocene or early Miocene) was built up in the western part of the area. It was about 40 miles across and has a maximum thickness of 3,000 feet. The lowest member of this series is a quartz latite near an andesite. It is overlain and in part interlayered with thin flows of rhyolite and that is, in turn, overlain by a latite. Next follows a thick body of pyroxene andesite and finally local bodies of bedded andesite tuff.

The Potosi volcanic series (Miocene) was the next important group of eruptions and followed the Silverton series after a period of erosion. It is the thickest and most widespread of all the series and it occupied an area of about

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[1)] Published with the permission of the Director of the United States Geological Survey.

13,000 square miles and had a volume of about 5,500 cubic miles. It has been divided into six members--three andesitic and three rhyolitic. The succession is given below beginning with the oldest.

(1) A great mass of andesitic rock erupted from many centers and forming a great dome-like mass over 100 miles across and several thousand feet thick. It is made up predominantly of hornblende and pyroxene andesites but varies from rhyolite to basalt. In the main part of the mass it is made up of irregular lava-flows, none of which are very widespread, and pyroclastic material, but near the borders it is built up chiefly of bedded, water-laid material which grades outward into gravels and arkoses derived chiefly of pre-Cambrian rocks. It occupied an area of about 10,000 square miles and had a volume of about 3,000 cubic miles.

(2) A period of erosion.

(3) Three low domes made up of rhyolitic-rock, partly in widespread regular flows, partly interbedded-tuff. The rocks have little quartz and much tridymite. The rhyolites carry biotite as the only important dark mineral while the tridymite latites carry biotite and augite. They occupied a total area of about 3,800 square miles and had a volume of about 600 cubic miles.

(4) Two volcanic cones and one larger more complex mass of andesitic rocks. They occupied an area of about 1,600 square miles and had a volume of about 100 cubic miles.

(5) An interval of erosion.

(6) The largest of all the rhyolitic masses. It formed a low dome or plateau over 100 miles across covering an area of about 10,000 square miles and having a volume of about 1,100 cubic miles. By far the greater part of the mass is made up of thick, widespread flows and tuff beds of a tridymite--quartz latite that carries abundant phenocrysts of plagioclase, quartz, orthoclase, biotite, hornblende, and titanite. In the extreme northwestern part of the dome the quartz latite is underlain by several hundred feet of tridymite rhyolite in regular flows and tuff-beds with some associated volcanic cones of rhyolite.

(7) Two volcanoes of andesite occupying a total area of only about 1,000 square miles and having a volume of about 160 cubic miles.

(8) An interval of erosion.

(9) The last of the Potosi eruptions were rhyolitic and covered an area of about 4,000 square miles and had a volume of about 600 cubic miles. The generalized section from the bottom up is: (1) A cavernous tridymite rhyolite in thick flows that filled in the gulches and canyons; (2) local bodies of latite-andesite associated with rhyolite tuff; (3) widespread, regular thick flows of tridymite latite; (4) local bodies of andesitic rock; (5) quartz latite tuff with some local flows, and grading into (6) widespread thin regular flows of quartz latite.

The Potosi eruptions were followed by a period of erosion. Over the new mountainous topography latite-andesites were erupted from several rather widespread centers. They are in local chaotic flows and breccias. They vary considerably in chemical and mineralogical composition but are characterized by the large size of their phenocrysts.

After penaplanation the Hinsdale volcanic series was erupted. By far the greater part of this series is made up of basalts and andesites with a basaltic habit in thin widespread flows. They formed a number of broad domes of which the one to the southeast, chiefly in New Mexico, was the largest and was nearly 100 miles across. These domes may have coalesced so that nearly the whole of the mountains, excepting possibly the western part, was covered by the basalts. They resemble plateau-basalts on a small scale.

Locally the basalts are underlain or interlayered with rhyolites. In the extreme southern part of Colorado the basalt is underlain by a volcano of latite-andesite and by gravels that increase in thickness to the south in New Mexico.

The latest eruption was a local body of andesite that came over a topography of Quaternary age.

Following the eruption the area was tilted from the center toward the east and north.

Among the interesting features of the area are the great bodies of rhyolitic rocks. They are predominantly quartz latites but near the rhyolites in composition. They resemble plateau-basalts rather than the usually described rhyolites in the regularity, flatness, and widespread distribution of their flows and in the lack of ordinary centers of eruption. Some of the domes were from 50 to 100 miles across and the maximum thickness was only a few thousand feet. In the canyons the alternating flows and tuff-beds look like alternating beds of sandstone and shale.

It is clear that the lavas were not superheated as nearly all were erupted with abundant and varied intratelluric crystals up to 40 per cent. They do not appear to have been high in mineralizers as they are not highly vesicular and the glassy bases of the flows have little water. The gas was retained until the lavas were practically at rest and appears to have escaped briefly after the groundmass had begun to crystallize.

The andesites, on the other hand, except for the Hinsdale andesite basalt, were erupted from local centers and both the flows and the tuff-beds are irregular, commonly chaotic.

The silica minerals of the rocks are interesting. Quartz is not very common as phenocrysts in the rhyolitic rocks and occurs also as phenocrysts in the andesites and basalts. It is also found in the rhyolitic groundmass of the rocks. Tridymite is about as abundant or more abundant than quartz and is found chiefly in the more porous parts of the groundmass or in the spherulites of the rhyolitic rocks. It is also common in the cavities of the andesites and basalts.

Cristobalite is a fairly common but rarely an abundant mineral and is confined to spherulites perched on the walls of the larger cavities of the andesites and basalts. It is very rare in the rhyolites and is confined to spherulites on the walls of large gas cavities. Clearly, the tridymite and cristobalite were crystallized as metastable forms.

Harvard University,
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VOLCANO RESEARCH OF THE UNITED STATES GEOLOGICAL SURVEY, 1928-29

T. A. Jaggar

(Abstract)

The Hawaiian station in 1928 recorded 1,034 local disturbances on the seismograph. Possibly this heralded the outbreak of Halemaumau February 20, 1929. On that day lava gushed up along a rift in the lava-floor and the base of the talus-slides. It was a pumiceous basalt spouting up in a jet for 200 feet. Fountains bubbled up along the crack, spread out in a lake, and filled the floor-cup. They continued in action about 37 hours. About 3,600,000 cubic meters of lava flowed in.

The outbreak was immediately preceded or accompanied by tilting of the seismograph, small shocks, and tremors. The phenomena strongly suggest that the under magma exerts a slow, massive push upward unvesiculated; and that when the crater-floor is reached, this material suddenly effervesces, and the effervescence spreads with heating and expansion from above downward. When the gas-ebullition reaches its limit, outflow, freezing, and shrinkage reverse the radial tilt, effervescence ceases, and deep jarring likewise ceases.

The eruption of 1929 was in accordance with expectations. A plotted curve of eruption cycles had indicated for Halemaumau a recurrence about the winter of 1928-29. An eruption of Mauna Loa is highly probable before the end of 1930.

Work at Lassen, California, and in the Aleutian Islands and Alaskan Peninsula has been continued. New seismographs have been installed and others constructed. A large area in Alaska has been explored geologically, studied, and mapped. A program for future work is being developed. (Abstract by the Secretary of the Section of Volcanology.)

Hawaiian Volcano Observatory,
Volcano Post-Office, T. H.

GENERAL MEETING

(Washington, D. C., May 2, 1930)

THE UTILITY OF GEOPHYSICS--GEODESY

Henry G. Avers

Geodesy is generally defined as the science that treats of the shape and size of the Earth. This definition was given in the early stages of the development of the science and embraces only the activity with which it originated. In the past 100 years, geodesy has been highly developed and its results have been of great value in the solution of certain problems connected with the Earth and, in addition, the data secured have a practical value in many lines of human activity.

The dimensions of the Earth are known with an accuracy sufficient for all practical purposes. A mathematical figure of the Earth, a spheroid, has been adopted which is a close approach to the actual figure of the Earth called the geoid. This figure forms the basis of all extensive operations in surveying, mapping, and charting in which the curvature of the Earth must be taken into consideration and it furnishes the means for the mathematical definition of points on its surface. Without a knowledge of the shape and size of the Earth, it would not be possible to place the features carried by maps and charts in their proper relative positions.

One of the most important branches of geodesy is triangulation. By means of triangulation distances between points and the directions of lines joining them can be determined with great accuracy. When the triangulation is properly connected with astronomic stations the latitudes and longitudes of the points and the azimuths of the lines can be computed.

The progressive nations of the world recognize the value of triangulation and have extended networks of arcs over their territory. In the United States about 27,000 miles of first-order triangulation and traverse have been run. This work fixes the geographic positions of thousands of selected points which are marked in a substantial manner that will insure their permanency for years to come. In addition to the first-order work there is a large amount of triangulation of lower orders along the coasts and in the interior of the country which has been used in charting and topographic mapping. It is planned to extend the present network of triangulation in such a way that eventually no point in the country will be more than 25 miles from a first or second-order triangulation-station.

Triangulation has been used extensively in the delimitation of political boundary-lines. Two notable examples are the boundary between Canada and the United States and that between Canada and Alaska. It has also been used in the location of certain state-boundaries and in fixing the limits of smaller political subdivisions. Its use in fixing the boundaries of public and private property is increasing. In using triangulation for these purposes, or for the reference of any survey, the perpetuation of the points is assured since in case of their destruction they could be accurately reestablished from undisturbed triangulation-stations even though the latter might be at some distance away.

The topographic maps of a country depend on triangulation for their proper coordination. Without this control there would be gaps, overlaps, and offsets where two maps meet. There is a growing demand for accurate topographic maps

by those carrying on the great industrial development of our country. The topographic map supplies valuable data needed in the efficient planning of lines of transportation and communication, in the flood-control of rivers and their development for navigation, in the construction of power-houses, dams, irrigation-ditches, and other engineering works.

A number of our cities have extended triangulation supplemented by traverse over their areas for the control of assessment-maps.

Another important branch of geodesy is the determination of the elevations above sea-level of many bench-marks distributed throughout the country. About 60,000 miles of first-order leveling have been run in the United States and there are many tens of thousands of miles of levels of a lower order which have been connected with the first-order net. The elevations secured by leveling furnish data of immense value in nearly all kinds of engineering operations and they are also used by the topographic engineer in the determination of the elevation of topographic features and in placing contours on the topographic maps.

The determination of the value of gravity is another important branch of geodesy. Already over 300 stations have been established in this country and the results are used in practical work in the physical and chemical laboratories and by the electrical engineer. Values of gravity are also used by the meteorologist in interpreting data necessary for making weather-charts and in the prediction of the weather.

A scientific product of geodesy, which indirectly has a practical value, is the proof of the isostatic condition of the Earth's crust. The geodetic data show that at a depth of about 60 miles below sea-level prisms of the Earth's crust of equal cross-section exert the same weight of pressure. This is a matter of great importance in the determination of the shape and size of the Earth. It is only through the application of the isostatic principle that a proper interpretation of gravity-anomalies and deflection of the vertical data can be made. Gravity and deflection of the vertical data may be used in showing the presence of abnormally heavy or light material in the Earth's crust near the stations. Gravity-observations are now being made by some of the large oil companies to aid them in their search for oil-deposits.

Geodetic methods have been employed to determine the degree of stability of the Earth's surface in a region that has been subjected to earthquakes. Triangulation in California, which had been executed thirty years or so ago, has been repeated and the differences in the geographic positions of the triangulation-stations in this interval have been determined. New arcs of triangulations have been run which will be repeated later. Lines of levels have also been run in this region with a view to discovering whether there are changes in the elevation of the ground between earthquakes and also to determine how much the Earth's surface has moved up or down during an earthquake. By comparing the results of the first and subsequent triangulation and leveling, it will be possible to determine to what distances from an earthquake-fault the effects of the quake extend. The study of earthquakes is an important one from an engineering standpoint for it may be possible to collect data useful in planning aqueducts, dams, and other engineering structures.

The practical value of the results of geodetic operations is evidenced by the many demands for data from the engineering profession. These demands have reached such proportions that the present Congress, upon the recommendation of the President, is increasing the funds for the geodetic work of the Coast and

Geodetic Survey more than threefold.

U. S. Coast and Geodetic Survey,
Washington, D. C.

THE UTILITY OF GEOPHYSICS--SEISMOLOGY

N. H. Heck

I do not approach the subject of the utility of seismology in a defensive mood, since this is entirely unnecessary. In the past the loss of life and property due to earthquakes has been great and the increase has been in a geometric ratio as the size and complexity of cities have increased, so that even moderate savings resulting from seismological investigations would be distinctly worth while. At first thought the problem would appear to be a purely engineering one, and while this is in the main correct the engineer must have fundamental data and methods of attack of the problems which it is the function of the seismologist to supply. I will illustrate by a recent example from the Panama Canal. It was decided that, in the design of the proposed Madden Dam which is to hold back the flood-waters of the Chagres River so that the level of Gatun Lake can be raised during the dry season, earthquake-effects should be considered. The advice of the United States Coast and Geodetic Survey was asked. It was realized that the engineering principles have not yet been developed in a satisfactory manner but it was found that in a still greater degree the required information regarding earthquakes in the region was not satisfactory even though the record is probably longer than that for any other place on the American continents.

Another well-known fact was emphasized--that our knowledge of the behavior of earthquakes in the central region of a strong earthquake is in a most unsatisfactory state. It was found necessary to estimate the probable conditions from those of other regions and we are far from sure that this is justifiable.

We must now endeavor to define the dividing line between the respective fields of the seismologist and the engineer. This is rather a live subject at the present time and it will be discussed at some length next Monday at the joint meeting of the Section of Seismology of the Union and the Eastern Section of the Seismological Society of America at the Bureau of Standards. When it comes to simulating earthquakes for the purpose of determining effects on structures, it seems that the development of the apparatus and its operation is the field of the engineer while the determination as to whether or not it accurately simulates the earthquake-movements is the field of the seismologist. Certainly the effects on structures and the design of structures to resist earthquakes are purely engineering matters. It is in these fields that interest is rapidly developing in this country. The view that will probably be accepted as to proper procedure will probably be intermediate between that now adopted in Europe and in Japan. In the former there are few destructive earthquakes and in general the studies are purely seismological. In Japan, with its not infrequent destructive earthquakes, naturally the main interest is in how to avert or reduce the destruction though there is full appreciation that little progress can be made without full understanding of the nature of the phenomena.

In using the word utility in the title I do not believe that the thought of purely commercial utility was in mind. We build great telescopes to study the stars and the results are of value even if there is no financial return

whatever. After all the study of the interior of the Earth is an astronomical matter but in this case the earthquake-wave takes the place of light and the seismometer thatof the telescope. Among the useful products of the investigation of the interior of the Earth by seismological methods are included: Knowledge of the nature of the outer layers of the Earth's crust, with detailed geological information resulting from seismic prospecting as a valuable by-product; knowledge of the interior where the conditions are very different from anything known on the surface.

The most recent views as to surface-conditions have been developed by Jeffreys. He arrives at several layers separated by layers of discontinuity and for the benefit of those in other fields than seismology I will read a brief recent statement by Jeffreys himself, which expresses in concise form exactly what his views are: "When many good seismological stations exist within about 1,000 km of the epicenter of an earthquake their records can be used to give information about the upper layers of the Earth's crust. It was discovered by A. Mohorovicic in 1909 that in such cases the records show not only the P and S of ordinary seismology, but also a pair of compressional and distortional waves that have traveled in an upper layer; their velocities are lower, but their amplitudes greater. Further work by Gutenberg, Conrad, and the present writer has shown that three layers are really concerned, which probably correspond to the granitic, basaltic, and ultrabasic layers of geologists. The foci in all cases yet investigated have been in the uppermost, or granitic, layer. Two waves travel in this direct from the focus to the observing station; these are denoted by P_g and S_g, and their velocities are about 5.4 and 3.3 km per second. Others called P^* and S^* seem to be transmitted down into the intermediate layer, travel along in this, and come up again to the surface. Their velocities in the intermediate layer are about 6.3 and 3.7 km per second. Others go right down into the deepest layer. These are the ordinary P and S. Their velocities are 7.8 and 4.35 km per second. Thus six distinct pulses are recognizable on the seismograms.

"The differences between these phases indicate that the granitic layer is about ten km and the intermediate one about 20 km thick; these estimates agree with those made by other means.

"The velocity of compressional waves in the uppermost layer agrees with that inferred for granite by L. H. Adams and E. D. Williamson from laboratory measures of its compressibility and density. That for the intermediate layer agrees with Adams' and Gibson's experimental value for tachylite, or vitreous basalt; and that for the lower layer with that of the same authors for dunite, an ultrabasic rock consisting mainly of olivine."

I should add that the recent studies in southern California have shown that Jeffreys' theory gives better agreement with the observed data than any other.

Another matter of very great interest is the difference in the transmission-time of the surface long-waves between suboceanic paths especially in the Pacific and continental paths. The speeding up amounts to at least twenty per cent. The theory of isostasy calls for an increase of density under the Pacific of three per cent over the continent. Increased density alone would tend to decrease the velocity but increased density is usually associated with a much greater increase in elasticity, and therefore the result is an increase in velocity. So far as I know no effort has been made to find out whether the increase in velocity corresponds to the increase in density.

An interesting question in this connection is whether the transmission beneath the Atlantic and Indian oceans is similar to that of the Pacific or the continents or intermediate between them. It has been generally held that it is intermediate in that the condition of the Pacific is associated with the removal of the moon from this part of the Earth's crust. European seismologists generally accept the view that the conditions are different but there is reason to think from some recent studies in this country that the difference between Atlantic and Pacific conditions is negligible and that the misapprehension has been due to the fact that Europeans have had very few accurate records of transmission over all oceanic paths under the Atlantic to work with. We are beginning to have a good many such records in this country and there are indications that there is no difference between the Atlantic and the Pacific. If this is proved to be the case there are likely to be interesting effects on the theories of the history of the Earth and moon.

So far there has apparently been no relation found between seismic waves and the depth of compensation derived from gravity-observations a little less than 100 km. For the present the only thing that can be certain is that all earthquakes should occur above this depth. While nearly all seismologists agree that earthquakes occur at a much less depth than this, Professor H. H. Turner, who issues the International Seismological Summary, has given a list of 78 earthquakes from 1916 to 1923 which appear to have had exceptionally deep foci, perhaps on the order of 100 km. However, the facts regarding these earthquakes are not yet determined with absolute certainty.

The utility of knowledge of the interior of the Earth is not so evident. However, the physicist, while making a most determined effort to carry his experiments outside of the field of existing conditions at the Earth's surface, is undoubtedly held within certain limits. It seems very doubtful if he can ever approach the conditions found in the interior of the Earth and facts regarding this must be learned through indirect methods. The principal evidence for the existence of a core, probably of iron and nickel in a liquid state, is the failure of transverse-waves to pass through. This fact as well as the fact that there is a surface of discontinuity 2,900 km beneath the surface of the Earth is derived purely from seismological studies. It is hard to conceive just what we mean by a liquid under great temperature and pressure and with a density of more than ten.

Returning to the surface of the Earth the artificial earthquake produced by explosives has been found very useful in telling about the characteristics of those layers nearest the surface. While it is no longer customary to rely on any single method of geophysical prospecting, seismic prospecting has become of considerable importance and a number of salt-domes have been found by this method. While the actual details of such a formation are best worked out by gravity-methods, the actual finding can ordinarily be best accomplished by seismic methods. The earlier method was to use a very large amount of explosive which made it very expensive not only through operating cost but also because of heavy claims for damage. Also, only the first wave recorded. More recent methods involve more sensitive instruments and continuous recording. This permits the use of smaller amounts of explosive and better interpretation. An interesting example reported at the recent meeting of the American Institute of Petroleum Engineers at its New Orleans meeting was the finding of two salt-domes beneath the waters of the Gulf of Mexico. There the seismic method is the only one that could be employed. A moderate amount of explosive was lowered to the bottom and fired and the receiving was done on board anchored boats. The periods are so short that effect of movement of the vessel could be eliminated.

There are obvious ways in which this method can be extended to learn geological facts of importance. An application already made has been the measurement of the thickness of an ice-sheet, and there are many cases where knowledge of depths of sediments is important.

Returning to earthquakes, it is obvious that since earthquakes are the results of slipping along a fault, in the case where there are sufficient earthquakes the fault-line may be traced even though covered by a bed of sediments. The hope of predicting earthquakes probably lies in finding out what is going on in a region along known faults which have been associated with severe earthquakes.

An outstanding problem is to find just what occurs in the central region of an earthquake, information of value both to the seismologist, who wishes to know how earthquake waves are developed and propagated, and to the engineer who has to design his structures to resist the conditions in the central region. It may be that there is an earthquake of such great intensity that nothing can resist it, but even in such case it is important to note just how large the region is over which we may expect such conditions. It is probably quite possible to build structures for all lesser conditions and they would stand all ordinary earthquakes. This is not a problem for crude, cheap instruments as has often been suggested but is rather one where the method of the seismologist must be used, and we should have instruments which should be just as capable of accurately recording the conditions as are the sensitive seismometers which go out of commission on the arrival of strong earthquake-waves.

There are other developments which I might discuss which would serve still further to illustrate the utility of seismology but I do not consider this necessary. Seismology then has utility which is almost unlimited in the case of preventing damage due to earthquakes and the seismologist and engineer working together can accomplish much. Seismology makes it possible to add to astronomy the study of the interior of the Earth itself. Theories are rapidly developing in regard to the portion nearest the surface as well as of the central portion of the Earth. Artificial earthquakes are telling much about the geological structure which is concealed to the eye. Seismology told us the nature of the earthquake-wave and its propagation with great completeness in the area outside the central region of an earthquake. It has before it a very useful field in determining in detail what goes on in the region of destruction, a matter of interest to the engineer and seismologist as well as the general public.

U. S. Coast and Geodetic Survey,
Washington, D. C.

THE UTILITY OF GEOPHYSICS--METEOROLOGY

W. J. Humphreys

The greatest poet of all scientists, and the ablest scientist of all poets, summed up a large part of the philosophy of life in a single one of his famous quatrains:

"A book of verses underneath the bough,
A jug of wine, a loaf of bread--and thou
Beside me singing in the wilderness--
Oh, wilderness were paradise enow."

Here the cultural and the aesthetic are typified by the book of verses, while the "thou beside me" notes the contentment in comradery. But, though enraptured as he evidently was while envisaging this paradise, Omar did not lose sight of the fact that, dream as we may, life is real--that while love and poetry soothe the soul and fire the mind, the stomach at least requires more solid food. And as another poet-philosopher has put it, we can somehow get along without every sort of luxury, but we must have something to eat. There is then ample justification for talking about the practical, or bread-and-butter, aspect of any science, even so aesthetic a science as that which treats of the billowy cloud, the blue of the sky, the purple of twilight, the rose of dawn, the bridge of the gods, and the golden rings that girdle the Sun and the moon.

Of course the inestimable cultural worth of the science of the air is admitted by every one, but what of its material value? What aid is it to the industries--the occupations that provide us with all the necessities of life and most of our luxuries? Well, it helps in so many ways that it is impossible to refer to more than a fraction of them, and that but briefly.

Climate--The climate of a place is the history of its past weather, a history which for convenience we commonly express in terms of tables, charts, and graphs. They tell us in concise form, and for the place in question, what has been the average monthly, seasonal, and annual precipitation; the prevailing direction and average strength of the winds, season by season; the average date of the last killing frost in the spring of the year, and the first in the autumn; the summer and winter percentages of sunshine; and a variety of other important facts. All this is useful because here at least the adage that history repeats itself is substantially true. From such data the physician can select places adapted to the needs of his patients; the manufacturer can, in many cases, wisely locate his factory; and the agriculturist can plant to best advantage. A fruit orchard, for instance, should be set out where late spring and early autumn frosts rarely occur, and not where both are frequent. In hilly regions such contrasting climates often pertain to places less than a mile apart. The contrast in the resulting business is even greater than that of the climates--success in the one case, failure in the other.

From this weather-history we know the most promising places for testing foreign fruits, trees, and plants; and also what can and what can not be successfully grown at any given place--information that could not be gained half so reliably in even a lifetime of guess and try. In this way and no other did we know at once where to plant the tung-tree, where to grow the Madagascar rubber bush, where the finest dates would thrive, and so on through a long list of successes.

Snow-catch, power, and irrigation--One of the things most desired of the meteorologist by the public is seasonal and other kinds of long-range forecasting; and here too, as in most other cases, a certain law of supply and demand holds, the law that "supply is proportional to demand." Please note the fact that nothing is said about the quality of the supply. In the present case it usually is worthless, as furnished by almanacs, or inferred from groundhogs, bees, beavers, the moss and bark on trees, or the state of the weather on some saint's day. Even cycles, of which many more than a hundred have been advocated, seem nearly always to mark a sharp division between regular, and hopelessly irregular, weather--regular during the period of the data examined, erratic ever after. How wonderful these cycles would be if only the tide of time would flow backward!

There is, however, one form of seasonal forecasting that is tolerably reliable and immensely useful. It is the foretelling of the amount of water that will be available during the summer and autumn for power and irrigation. This is based on snow surveys over various water-sheds, from which the subsequent run-off can be computed. Even where direct surveys are not practicable the approximate amount of available snow can be estimated from the record of the number, duration, and intensity of the winter's snowstorms. This bit of applied meteorology is very helpful throughout nearly the whole of the western one-third of the United States, as it obviously must be wherever water is at a premium for power and for irrigation.

Flood-warnings--Another branch of meteorological work that involves a greater or less long-range feature is the warning of floods. From a lot of things, especially the capacity or incapacity at the time of the soil to absorb more water; extent, depth, and location of the snow-covering; temperature of the air; amount of rainfall and the up-stream stages, it is possible to foretell with great accuracy what the down-stream stages will be some time later--a few hours to a day later in the case of short swift streams; a few days to a week for most rivers; and even two or three weeks for such places as the lower Mississippi. The practical value of this application of meteorology in the protection of property and the saving of lives is exceedingly great. Besides, this branch of the service does not have to await the coming of a spectacular, first-page flood, for to each of these there are hundreds of lesser ones that together require continuous attention, year in and year out.

Fire-weather--The antithesis to the flood is the forest-fire, and here too meteorology comes in with an abundance of practical help. Not only is a knowledge of the coming weather for a day or two in advance often of great help in fighting fires after they are started, but a knowledge of the humidity and winds, both actual and prospective, materially aids in preventing fires. When the humidity is so low that the duff and other combustibles are just so much tinder the forest-ranger makes every preparation for instant and vigorous work in suppressing any fire that may get started. Sawmills shut down work lest fires may start from them, and logging camps guard them in every way practicable against the dread danger.

Frost-warning--Another specialized but highly practical use of meteorology is the warning of heavy frosts, both in the blossoming season and at harvest time. When the low temperature is owing to clear skies and still air--when, in short, the temperature of the air is markedly lower near the ground than it is 50 to 100 feet above--protection from frost by artificial heating is not only possible, but in many cases economically practical. If, however, the low temperature is not associated with an inversion, as just described, but is due to the importation of a deep mass of cold air artificial heating on a large scale is well-nigh impossible, and certainly uneconomical. When there is a marked inversion of the radiation-type the orchardist should save his crop; when there is no inversion and the low temperature is owing to the importation of cold air he should save his fuel. Clearly, too, as orchard heating is expensive, and loss of crop disastrous, the grower must have reliable information. To heat when heating is unnecessary is nearly as bad financially as not heating when it is necessary. This service is a very specialized part of applied meteorology, highly reliable, and of very great economic value.

Fruit-spraying--Not only frost may injure fruit but fungus too can spoil it, and so the custom in many commerical apple orchards is to spray with such material and at such times as will prevent the occurrence of scab. These times are determined by two things, the presence of the spores and the state of the

weather with respect especially to humidity and rain. But spraying is laborious and costly, and therefore useless spraying, that is, spraying when the weather of the next day or two will nullify the work done, most exasperating. In this case, too, the orchardist urgently needs ample and reliable foreknowledge of the early coming weather.

Harvest-weather--Still another practical application of meteorology is the informing of hay-makers and other harvesters of the coming of suitable weather for their work. "Make hay while the sun shines" is absolutely essential in the harvesting of alfalfa, almost as necessary in the curing of red clover, and highly desirable for grasses of all kinds. And there are other crops, too, that can not stand a wetting while being cured. Some of these are sun-dried raisins, prunes, and peaches. An unforeseen rain when acres of raisin-trays are out would be a mighty bad "slip 'twixt the cup and the lip." Neither is undue caution practical, for piling up and covering thousands of trays is not only laborious and expensive but also it delays the drying. Whatever the harvest, then, to which sun-curing is essential and rain injurious, a necessary element in its success is the proper use of meteorological observations and information.

Cold-wave warnings--Not infrequently throughout the year except, perhaps, midsummer, unseasonably cold air spreads from higher to lower latitudes, generally with a marked eastward component. In the United States these "cold waves" often are very intense and lead to much discomfort and at times to considerable loss of produce and injury to other property. Hence in many cases warnings are issued not only to the general public, enabling them to protect water-pipes, automobiles, and many other things subject to injury, but also, and often for longer periods, to shippers whose produce, from onions to iron ore, needs to be protected from the cold. Freezing spoils an onion and a spoiled onion is even in the way, one does not want it around; and frozen iron ore (wet when loaded or rained on afterwards) also is in the way because the cars can not be unloaded and therefore clutter up the freight yard. These two commodities may be extremes in some respects, but they are like a great many others in requiring careful attention on the part of their shippers to the weather-conditions to be encountered while en route from point of shipment to place of destination. Yes, the shipper makes great practical use of meteorology.

Storm-warnings--The shipper by water, no less than the shipper by land, is deeply concerned in the weather, but more especially in the occurrence of heavy winds. Hence at every important shipping port all available weather data and the latest forecasts are at the service of those who are coming in or preparing to sail. If the storm promises to be severe many vessels, especially the smaller varieties, keep behind the breakwater, or in some other sheltered spot, until the storm has spent its force. In this way vast property-interests are protected and innumerable lives saved, as well as greater comfort assured, and reasonable insurance-rates obtained.

Aviation-forecasts--But travel and commerce is through the air as well as over water and land, and here meteorology has its most urgent application. For most purposes it is quite sufficient to be told that there will be local showers in northern Ohio, for instance, this afternoon, but that does not meet the aviator's needs. He wants to know whether there will be a thunderstorm over the Cleveland airport at 3 o'clock sharp, the exact time he is due to arrive at that particular place. He may not, as a rule, need to have very much weather-information, nor need that little long ahead, but what he needs he needs, and he wants no may-be about it.

In all the above varied lines of endeavor applied meteorology clearly plays

an important role, as it does also in practically every other occupation from a to z--in aviating, as just explained; building, cement must not freeze before it sets; canning; dairying; eating, eat sparingly in hot weather; farming; grazing; haying; imbibing, drink temperately, too, when the weather is hot; japanning; kilning; lumbering; manufacturing; nutting; observing, one can not see the stars through a cloud; painting; quadrilling, dancing is nicest when the air is cool and dry; reading; sailing; trading, "do business with men when the wind is in the northwest"; umpiring; varnishing; walking; x-raying; yachting; zoologizing, some animals are out most when the air is hot and dry, others when it is cool and damp. And so it goes from beginning to end of the alphabet and back again, and would if that alphabet were twice as long, for we ourselves and all the things we do are affected by the state and condition of the weather.

"What is it moulds the life of man?
The weather.
What makes some black and others tan?
The weather.
What makes the Zulu live in trees,
And Congo natives dress in leaves
While others go in furs and freeze?
The weather."

U. S. Weather Bureau,
Washington, D. C.

THE UTILITY OF GEOPHYSICS--TERRESTRIAL MAGNETISM AND ELECTRICITY

D. L. Hazard

The importance of pure science as a complement to its practical applications is becoming more generally recognized every day. Additional examples of the use of the results of research in an unexpected direction are steadily accumulating so that it is not safe to assume that any abstract bit of knowledge may not sooner or later be put to some concrete practical use. It will not be out of place, therefore, to pause for a few moments at this time and see along what lines these uses have developed, partly as a possible guide to future progress, but more particularly as a stimulus to the student of pure science, who frequently finds difficulty in securing support for investigations which do not appear to lead directly to practical results.

In the case of the magnetism and electricity of the Earth brief mention will be made of the importance of knowledge regarding them to the mariner, the land surveyor, the aviator, the traveler, the prospector for ore or oil, the operators of telegraph and cable lines, the radio engineer, and the inhabitants of regions of electrical storms.

Everyone knows in a general way of the use of the compass as a means of determining direction, but many do not realize that it derives its directive property from the Earth's magnetism and comparatively few are aware that the compass-needle does not point true north and that the angle which it makes with the true meridian is different at different places and at different times.

From the date of its invention (as early as the 13th century) to the present time, the mariner's compass has been indispensible to the navigator as a means of maintaining his course when known landmarks and heavenly bodies can not be seen; and today practically every vessel of every size and description

is equipped with one or more compasses. Even the limited number of larger steamers equipped with gyro-compasses carry magnetic compasses also for use in emergency. In order to lay his courses correctly the navigator must know for the parts of the world which he is to visit the direction of the compass-needle with respect to the true meridian, since all modern charts are laid down with reference to the true meridians and parallels. To supply this information observations of the Earth's magnetism must be made in all parts of the globe, on land as well as at sea, and repeat-observations must be made from time to time at a sufficient number of places to determine how it is changing with lapse of time.

The surveyor's compass was one of the earliest forms of surveying instruments, and for nearly all of the early land surveys in the United States the boundaries were defined in the deeds by compass-bearings. Even at the present time it is in general use where land values are low and more accurate and expensive methods of surveying are not justified. The surveyor attempting to retrace the lines of a tract of land surveyed originally by compass must know how much the direction of the compass-needle has changed between the dates of the two surveys. Here also, repeat-observations are required from time to time at selected stations distributed all over the country.

For the traveler in unexplored regions, particularly where forests prevail, the compass is still indispensible, and the not infrequent reports of persons being lost in forests of small extent show that it is a wise precaution to take a compass for even such short departures from the beaten path.

In the early days of flying, before special forms of compass had been devised, flying by compass was very uncertain, because of the effect of the high speed and sharp turns on the ordinary forms. Now, however, the compass is as necessary to the aerial navigator as to the mariner. While it is true that for much of the cross-country flying dependence is placed on mapped physical features of the route to be passed over, and on the more frequented routes beacons are being provided for guides at night, nevertheless, fog, rain, or clouds may at any time cut off the sight of these objects and dependence must then be placed on the compass.

For many years magnetic instruments have been used in the location and development of magnetic iron ores, by measuring departures from the normal distribution of magnetism in a particular region. In most cases, however, the ore-bodies and the resulting magnetic anomalies were large and measurements of great accuracy were not required. In the case of the use of magnetic methods in the search for oil, which has now become quite general, deeper seated and less highly magnetized geological formations are involved and the anomalies to be expected at the surface are much smaller, so that the instruments and methods used must be designed to give results of a high degree of accuracy. Provision must be made for taking account of the change of the Earth's magnetism in the course of a day's observations. In some cases this is done by making observations throughout the day at a base-station near the area being surveyed, sometimes the field observer repeats observations at a base-station several times during the day. In many cases, however, the ordinary changes in the intensity of the Earth's magnetic field during the day are not of sufficient magnitude to require consideration and attention need be given only to the unusual changes which occur at the time of a magnetic storm. Many oil prospectors in the south and southwest are taking advantage of the operation of the magnetic observatory at Tucson, Arizona, to secure prompt information on this subject. When the photographic record of a day's magnetic variations is developed in the morning, the observer can see at a glance whether a magnetic storm occurred

during the 24 hours ending at 7:00 a.m. When a storm is found to be in progress, telegrams are at once sent giving the day and hour of beginning and the strength--moderate, severe, very severe. After the end of the storm another telegram is sent giving the day and hour of ending. The engineers receiving these messages use with caution observations made during the progress of the storm and repeat such of them as may appear advisable. Some prospectors have arranged to be supplied with blue-prints of the magnetograms covering the stormy period, so that they may determine for what portions the departures from normal were of sufficient magnitude to require consideration. To make this information more generally available the Oil and Gas Journal, beginning with February 27, 1930, has arranged to publish reproductions of the magnetograms covering the periods of magnetic storms.

Most users of radio are familiar with the form of static associated with thunderstorms, which is directly due to the electrical discharge. In addition to this, studies of radio reception extending over a series of years have shown pretty definitely that the signal-strength is affected by variations in the Earth's magnetic field. In particular it has been found that, as a rule, a magnetic storm produces an increase in signal-strength for long waves and a decrease in strength for short waves. Two recent attempts of the National Broadcasting Company to relay European programs through short-wave transmission were defeated owing to the presence of magnetic storms.

For these studies a knowledge of the variations of the Earth's magnetic field is essential, and many of the agencies engaged on them are making use of the records of the magnetic observatories of the Coast and Geodetic Survey in one form or another. Recognizing the importance of a prompt knowledge of the occurrence of a magnetic storm in these and similar investigations, plans are being considered for broadcasting information on the subject.

There occasionally appears in the papers a statement that telegraph- and cable-lines have been put out of commission for a few hours, due to the occurrence of a magnetic storm, sometimes accompanied by an auroral display and an unusually large spot on the Sun. The interruption of service on the lines is directly due to currents of electricity in the Earth much in excess of those used in the transmission of messages, but these currents are so closely associated with the other phenomena that it is impossible to say which is cause and which is effect. The studies of earth-currents and atmospheric electricity are in their infancy and much more observational data is needed to develop the various relationships.

Lightning is the most outstanding manifestation of terrestrial electricity, and it is a very serious menace in some regions. Here again much more study is needed to determine what local conditions have an effect on its destructiveness and the best means for protecting buildings from its ravages.

With so many matters of everyday occurrence directly affected by the Earth's magnetism and electricity, it is very important that we should find out all we can about them, how they are distributed over the Earth's surface, how they change, how they originated, and what causes them to change.

U. S. Coast and Geodetic Survey,
Washington, D. C.

THE UTILITY OF GEOPHYSICS--OCEANOGRAPHY

Henry B. Bigelow

Oceanography, as a branch of science, has direct utilitarian sides, for (inter alia) it embraces all studies of the biology of animals that are the object of the commercial fisheries, and of the various events in the sea that affect navigation. The question whether a rational basis for forecasting the weather can be found in the variations that take place in the temperature of the sea is also much to the fore at present.

The importance of oceanography in the conservation of the sea-fisheries lies in the fact that the enormous supply of human food (also important by-products) that is yearly drawn from the sea is truly a natural resource, as contrasted with the yields of agriculture on land. Man has nothing whatever to do with its production, but merely takes a part of the wild crop that the pastures of the sea nourish. In making this statement, we do not ignore the fact that vast numbers, running into billions, of sea fishes have been artificially hatched, and liberated; but it is not likely that such attempts have had any appreciable effect upon the stock of any of the important food-fishes of the open sea. And while shell fish are cultivated to some extent, this industry is in its infancy.

It may be accepted as axiomatic that intelligent utilization and conservation of a wild crop requires as its basis a sound understanding of the natural history of the plants or animals in question. And this applies as truly to marine food-fishes as it does to forest trees. Although the supply of fish and shell fish in the sea be vast, fishermen and scientists alike realize only too well that the stock is not inexhaustible; the rapid depletion of certain species emphasizes this. And greatly though the extent of the ocean exceeds that of the land, all of the great fisheries (except for whales) are confined to the shelves and slopes of the continents in comparatively shoal waters. Even within these boundaries the grounds on which the important commercial species are plenty enough to support profitable fisheries occupy only a fraction of the area. No great fishery (again excepting for whales) has ever been developed, or is ever apt to be developed in the deeps outside the continental slope, the species living in the ocean-basins being too few in individuals, as compared to the population of the inshore grounds, and most of them too small in size. The whale-fishery alone leads out into the high seas far from land, and no increase in its yield can be expected; on the contrary, how to maintain the stock of whales is now a crying problem. In short, only a small fraction of the total area of the sea supports practically all of the species (and individuals) from which a mass-harvest can be hoped. The basic fisheries task then is to make the greatest possible use of the food sources of this peripheral belt of the sea that is compatible with avoiding the danger of over-fishing. And a second task, hardly less pressing at present, is to safeguard the industry against the disastrous effects of fluctuations that take place in the available supply of fish from natural causes.

Although the problems involved in these two cases are fundamentally distinct, a solution in each can be hoped for only from tracing out the lives of the fishes in question, especially their reactions to their environment, animate and inanimate. In other words, the disciplines of ocean biology must be employed, whether the aim be protection or prediction--any other line of attack is so haphazard as to be futile.

Whenever the fishery for one species or another increases greatly in in-

tensity, as happens periodically (it is now happening with the haddock), the immediate, practical task is to estimate the drain of the species which it may be expected to withstand. But this is an extremely complex question to answer, for it involves not only how many fish can safely be caught, but at what age it is wisest to catch and market them, on what grounds it is wisest to fish, and by what method. None of these questions are we yet able to answer for a single species; and all of them must be answered if conservation is to be intelligent, not simply hit or miss. To make this more clear, we need merely point out that as only a fraction of each year's crop can grow to breeding age in any case (else the sea would be solid full of fish), it may be wisest to utilize the smaller sizes in some instances, the larger sizes in others. For instance, we are totally in the dark as to whether the annual capture of a billion or more small herring in the Gulf of Maine, year after year, has had any effect whatever on the numerical strength of the stock of adults breeding there. Could a large catch of the latter be made there with equal impunity?

The question of where it is wisest to fish may be an important one in the maintenance of any given fishery, for it involves the interrelationship between the nurseries of the species and the outlying areas that these nurseries supply. To fish the former hard may be dangerous, but this may not apply to the latter. And "how to fish" is a biological question, as well as a purely economic one, because different kinds of gear take fish of different sizes, while the type of gear used may also determine the fishing ground frequented. The otter-trawl, for instance, can be used only on comparatively smooth bottom, the purse-seine only close to the surface and in smooth weather. Whether the grounds, depths, or seasons determined by the method adopted are wise from the standpoint of conservation can be settled only by knowledge of the life history of the particular fish. The effect that a fishery may have on enemy-species that are caught incidentally, or upon species upon which the commercial fishes prey, may also be a prime factor. Any method that destroys large numbers of the former may actually benefit the primary object of the fishery, even though the latter be hard fished at the same time. To disentangle the economic interrelationships of the different species falls directly within the province of the oceanic biologist.

When the yield of any fishery seriously declines, the industry is at once faced with the question whether this decline is the result of overfishing, or whether it reflects a decrease in the stock of fishes existing in the sea, resulting from natural causes with which the hand of man has had nothing to do. And when events of this latter category take place, the economic problem is to safeguard the industry, not the fish, against the unpreventable fluctuations in the supply of raw material. In general, fluctuations of this sort (of which many instances, often extreme, are now on record) reflect the fact that a year of highly successful reproduction is for many species of sea fishes, decidedly a rare event; and that when an abundant generation is reared in the sea--of mackerel, for instance, or of herring--it dominates the stock of the species in question for a long period thereafter; either until its members drop out of the picture from the natural death-rate, or until another large year class is produced. All this has been signalized so often in the literature of fisheries biology that no specific instances need be quoted here. What must be emphasized is that we know of no instance, up-to-date, where the stock of a species that has shrunk in this way, has failed to recover after a period of years, in spite of the drain that the fishery may have placed upon it. Perhaps the ups and downs of the mackerel off the coast of New England afford the most spectacular example of such recovery that has yet been followed.

To predict such ups and down would be of great economic value. We do not

yet know their exact causes, or how these causes work. But causes are discoverable, because nothing in the sea falls haphazard; and the oceanographer stands in the best position to make such discovery, and to forearm the fisheries (and the consuming public) against their often disastrous results. It is clear that for any sound understanding in this case, the whole life-chain of the species in question must be traced, link by link, which calls for critical and protracted investigations ramifying into chemistry and physics. Consequently, economics demand that many problems be attacked in the sea, which at first may seem remote from any practical application.

The greatest gain that oceanography could offer to navigation, and the only considerable economic gain to be hoped from it in this respect at present, would come through studies of ocean-drifts, of tidal currents, and of the depths of water along coasts not yet adequately charted. The need of a more detailed knowledge of the major ocean-currents is fully appreciated by the hydrographic surveys of the seafaring nations. To illustrate how generalized our charts of the ocean-currents are, even for the more traveled seas, one need only consider the vagueness of our knowledge as to the fluctuations in the geographic location of the inner edge of the Gulf Stream off the east coast of North America, and of the eddying movements that confuse the orderly procession of that body of tropic water toward the northeast. In less frequented regions "sketchy" fairly describes our present picture of the currents.

A better understanding of the underlying forces that keep the ocean-currents in motion is also needed;but this cannot be gained by continued compilation of log-reports, because the underlying waters are involved as well as the surface. Quite a different method is called for, one that finds one modern expression in mathematical analyses of dynamic factors in the sea, such as are now being actively undertaken at various places in Europe and in North America. Here successful application to the ocean demands cooperation by many institutions.

Serious gaps also remain to be filled in our knowledge of the tidal currents around the shore-lines of all the countries that have been backward in prosecuting tidal surveys. The paucity of detail as to the direction and velocity of these, as given in the sailing pilots for the South Atlantic, for the Central Pacific, and for the eastern Archipelago make this clear. Here a wide field lies open for oceanographic research, where knowledge gained will eventually be of direct practical advantage.

Similarly, while existing charts leave little to be desired for navigating purposes in most parts of the world, few laymen appreciate the extent of shore-line, off which present knowledge of the depth of water is still more or less imperfect. For an example we need seek no further than the east coast of Labrador, where soundings are not only so few, but many of them so inaccurate that a stranger must proceed with the greatest caution. The economic importance of a detailed knowledge of the depths in coastwise waters is self-evident. Now that sonic methods of sounding are in general use, the application of measurements of depths to navigation enters a new phase. A detailed knowledge of longshore currents is also urgently needed in planning harbor-entrances on sandy coasts, or constructions undertaken to protect shore property.

In introducing the question of seasonal weather-forecasting into this symposium, we must point out that there is as yet no general agreement whether, or to what degree, forecasts of the weather based upon any observable feature of the sea-water can be made reliable enough to prove of practical service, unless it be in especially favored regions. The immediate problem to be solved

is whether investigation of the interaction between sea and air does indeed offer reasonable prospect of yielding practical benefits. There is no reason to suppose that any study of surface-temperature of the sea, or of the variations in the ocean-currents, no matter how detailed, could assist the ordinary daily weather-forecast. The goal that some students believe attainable is prediction of the seasonal weather-character over the adjacent lands. And proof that industry as a whole would welcome such assistance is to be seen in the fact that many concerns have in the past been willing to pay high for long-range predictions, from various sources, though realizing that these are still in the experimental stage. It has often been suggested that at least a partial basis for such forecasts is to be found in the sporadic variations that are known to take place in the surface-temperature in various parts of the sea, combined with corresponding expansion or contraction of the ocean-currents, and with the rate of evaporation from the surface. This is now being actively tested at various centers, the crux of the matter being to establish whether, or in what parts of the ocean, temperature-abnormalities or other changes in the water do antedate alterations in the weather of the overlying air regularly enough to be used as a safe basis for forecasting. Nor can any general rule be assumed to apply in this respect, the whole question being an extremely complex one--so complex in fact and in a state of such rapid flux that I can no more than mention it here.

Museum of Comparative Zoology,
Cambridge, Mass.

THE UTILITY OF GEOPHYSICS--VOLCANOLOGY

A. L. Day

This address was presented informally and no manuscript was prepared.

THE ADVISABILITY OF GEOPHYSICAL INVESTIGATION IN THE ARCTIC BY SUBMARINE[1)]

Captain Sir Hubert Wilkins

In order to complete the information needed to construct a plan for comprehensive meteorological investigation in the polar regions and encouraged by experience and knowledge gained in serving for four winters and seven summers in the immediate vicinity of the Arctic Sea and with enough experience with submarines to realize their limitations, I propose to carry out an expedition to be known as The International Arctic Submarine Expedition Incorporated for the purpose of geophysical investigation on a route between Spitsbergen and the Bering Sea. In view of the forthcoming International Polar Jubilee 1932-33 I believe it is particularly desirable to carry out investigation with a submarine at an earlier date in order to know whether submarines might be used to economically enable various sections of investigators to reach the most favorable and otherwise inaccessible points.

Attention is drawn to the fact that there are no icebergs of great size within the Arctic Sea. No one has yet seen great unbroken ice-floes in that Sea during the summer months. Large pressure-ridges have not been observed except near the coast and the deepest of these do not extend for more than 100 feet beneath sea-level. The general thickness of the ice in the summer is not expected to be more than ten feet and because the arctic ice is so much broken in summer it is believed that 25 per cent of the total distance to be covered will be clear water and that a submarine will be able to come to the surface within ten miles of any given spot in the Arctic Sea.

The temperature of the arctic waters in summer is not expected to be lower than 28° Fahrenheit, and the air-temperature at the period of operation will range to 50° Fahrenheit.

It is proposed to obtain, if possible, an existing submarine and to carry out some trials and investigations in Spitsbergen or Canadian waters this season and to start the long journey from either Spitsbergen or the Bering Sea during the month of July 1931. If an existing submarine is not available it is proposed to place an order with Messrs. Vickers Limited of London for a new submersible vessel to be delivered June 1, 1931.

It is believed that complete navigation of such vessels by means of magnetic compasses will be possible, if necessary, but it is also believed that it will not, even though the summer months of the Arctic are cloudy, be necessary to navigate entirely by magnetic or gyro-compasses because the submarine will necessarily come to the surface frequently to recharge its batteries, and that astronomical and other scientific observations can be made at least every 50 miles en route.

It is believed that by progressing at a speed of not more than four miles per hour under the ice there will be no danger of damage to the submarine by collision. The submarine will be capable of operating for a total distance of 5,000 miles under water and for 100 miles without recharging batteries. It will be provided with mechanical devices which will enable the crew to reach the surface from under ice ten feet thick and it will be possible to penetrate ice 50 feet or more thick with cored mechanical drills and by that means obtain

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[1)] This paper was presented upon invitation of the Union.

air, discharge engine-exhausts, and recharge batteries even if it is not possible for the crew to come to the surface. While these mechanical arrangements will be provided, it is thought that the open leads of water and cracks in the ice will provide all access to the air that is necessary.

The vessel will be equipped with a compression-chamber through which men may emerge in diving apparatus even though the submarine is far under water. It will be equipped with an observation-chamber in the extreme bow and with finely focussed light-beams for aids in observing ahead and above the vessel. The vessel would carry all required arctic equipment for surface travel if necessary. Room will be available for an abundance of supplies and accommodation for all scientific equipment likely to be of service.

I request that members of the American Geophysical Union and of the International Geodetic and Geophysical Union advise me in relation to the scientific equipment required and the work to be done. I suggest that the geophysical investigations might include: (a) Meteorology in so far as determining whether it is advisable to maintain an observatory on the ice between latitudes 75° and 80° north, longitudes 150° and 170° west, and to take, en route, general meteorological and upper-air observations on the surface and by means of a captive observation-type balloon and free balloons; (b) measurements of gravity by means of the Meinesz gravity-apparatus; (c) hydrography by means of sonic and mechanical depth-finding apparatus; (d) oceanography including the collecting of water-samples and bottom-samples and by observing the temperature, force, direction, and animal-content of the ocean-currents at various depths; (e) terrestrial magnetism including the comparison of the standard magnetic with the gyro-compass at the geographic north pole (observations for vertical and horizontal components can be made together with other observations necessary while the submarine is stationary at the surface, charging batteries); (f) ice-distribution, its character and movement by means of direct observation and by photographs from the captive-balloons; (g) radio and television broadcasting-experiments carried out each day.

The submersible vessel will be manned by a crew of not less than twelve men including: Commander of the expedition; master of the submarine; chief scientist; scientist assistants; helmsmen (2); engineers (2); electrician-cooks (2); radio telegraphist; cinematographer; sound-engineer and, or, others depending upon the size and type of vessel. While these things are put before the American Geophysical Union as suggestions, the definite plans for the Expedition have not yet been laid down, and in the event of it being necessary to construct a new vessel it is perhaps possible to provide for any other form of investigation thought advisable.

The City Club,
New York, New York

SECTION OF GEODESY

(Washington, D. C., May 1, 1930)

THE VALUE OF GEODETIC SURVEYING IN CANADA

Noel J. Ogilvie

In the surveying and mapping of Canada, it is of prime importance that the surveys and the resultant maps should measure up to modern standards of accuracy. At the same time the cost of the surveys should be kept at a minimum. Where organization is efficient and skilled personnel using modern instruments and methods are employed, the expense involved in conducting a survey of high accuracy need not greatly exceed the cost of less exacting operations. Moreover, the compensating advantages of an accurate survey more than offset the extra expense involved. Where suitable geodetic control-points are used as a basis for the location of the topographical features and artificial developments throughout the country, the resultant map or chart may be made to comply with every requirement of accuracy and detail; and where suitable monuments are established in the field to perpetuate the control-points of the survey it is a simple matter to return at any future time to locate or survey local improvements or developments. Where the original surveys of a large area are not based on accurately located points forming part of a nation-wide scheme of geodetic control, the map of the district cannot be constructed within the limits of accuracy usually required, and as the needs of the country become more exacting in this respect it would be necessary, in all probability, to repeat the original surveys with the attendant expense and delay.

Where an entire country as large as Canada is ultimately to be systematically surveyed and mapped, questions relating to the control of cumulative errors in surveys must be decided and the surveys in all sections of the country made to correspond in accuracy. At the same time a system must be developed whereby the surveys of a particular locality may be accurately placed on the map with regard to elevation and geographic position relatively to the position of surveyed features in distant sections of the country. Failure to make provision for this condition of coordination results in overlapping or discrepancies in adjoining map-sheets when intervening gaps in territory already mapped have been finally completed.

Eminent geodesists, surveyors, astronomers, and mathematicians have in the past given these subjects much study. It has been found that as the accuracy of the survey is increased the cost is naturally somewhat increased. On the other hand, it has been found that unless special methods or precautions are taken to keep the errors within permissible bounds, they will tend to accumulate and the resultant discrepancy becomes larger and larger as the survey is extended across greater areas, till finally the accuracy of the whole scheme is so impaired that the value of the map as an accurate record is materially decreased.

In other words, the methods of survey which are suitable and sufficiently accurate for relatively local areas cannot be used over large areas without the introduction of some additional methods to prevent the cumulative errors from becoming too large. This principle can readily be understood by a consideration of the specific case of a man measuring with a yard-stick a linear distance of 30 miles. Long before he had completed the measurement he would have lost confidence in his ability to complete the undertaking in a satisfactory manner, involving as it does about 53,000 repetitions of the measuring unit. Similarly a surveyor extending a survey across Canada a distance of 3,000 miles, using a

300-foot steel tape would find it necessary to apply his tape about 53,000 times and the measurement of the total distance, so obtained, would in all probability be subject to grave error.

Years ago, a scheme was finally developed in Europe and later adopted by practically all civilized nations of the world whereby a system of choosing control-points at fairly regular intervals across the country was devised, their geographic positions being determined with great accuracy. A specialized system of surveying was developed in the establishment of these points whereby cumulative errors were not allowed to become appreciable. It will be readily seen that by the adoption of this method, any locality can be provided with one or more such points, the latitude and longitude of which has been found with great precision.

Afterwards in making the detailed surveys of each locality or district, these known points are used as centers from which to start and finish the local surveys. As the distance between the neighboring control-points is relatively short, gross errors in making the local surveys are quickly detected and corrected. Cumulative errors so inherent in surveys of this nature thus cannot become so large as to vitiate the accuracy of the whole survey or resultant map, and it is not necessary to increase the accuracy and therefore the cost of the detail survey. At the same time local topographical surveys tied to the nearest control-points and by this means accurately shown on the map with regard to geographic position will be also accurately delineated with regard to distant topography which has also been carefully tied to the control-points in that locality. The cost per unit of area of the control-survey is relatively small, and the total cost for mapping and charting any large area is found to be reduced to a minimum for any given standard of modern accuracy.

Canada has been fortunate in this connection in that she has profited by the mistakes of other countries and at a relatively early date has realized the economic importance of building her mapping and charting operations on a grid-work of geodetic control-points.

The establishment and position determination of these control-points involves a high degree of technical skill and knowledge of the mathematics of the Earth considered as a geoid or flattened sphere. During the last century the keenest mathematicians of Europe and America have been adding, little by little, to the perfection of this science, which is known as geodesy. The organizations carrying on work of this nature in various countries are known as geodetic or precise triangulation-surveys. Occasionally they are known by other names. The British Ordnance Survey, the Great Triangulation Survey of India, the Prussian Geodetic Survey, the United States Coast and Geodetic Survey, and the Geodetic Survey of Canada are all similar organizations in this respect. On the finished map or chart these control-points are seldom marked and the lay public does not realize that behind the compilation of the nicely lithographed sheet there exists, at intervals of 10 or 20 miles, control-points established with great precision which serve as a base or foundation upon which to build the actual map.

So far reference has been made only to horizontal control-points involving only latitude and longitude. In the making of topographical maps it is equally important to have control in a vertical plane. This may be defined as a series of points, the elevations of which are determined with precision. Through the medium of precise levelling, lines of levels are extended throughout the country from sea-coast to sea-coast. Commencing at a well-determined point at mean sea-level the survey is extended inland, and permanent monuments or bench-marks

of known elevation are established in each community. It is of vital importance for local surveyors or engineers dealing with drainage schemes and power-developments, or for the more extensive pursuits of the topographical map-makers to have at their disposal these points of known elevations from which to commence their local levelling determinations. Canada, up to 1930, has established bench-marks along 24,000 miles of precise levels and 20,000 miles of secondary level-lines.

Closely allied to the establishment of horizontal control-points by the methods of geodetic triangulation is the work of the geodetic astronomer in checking, by the determination of precise astronomical values, the twist or deviation in azimuth of the triangulation-nets. In the extension of nets of primary triangulation Laplace-points for the correction of twist are usually established in Canadian practice every 8th or 10th triangulation-station.

Astronomic position determination is made use of for two other purposes--for the establishment of political boundaries and for the determination of the deviation of the vertical in connection with the determination of isostatic compensation.

Provincial and international boundaries are usually established by the methods of geodetic astronomy, due to the fact that interprovincial and international agreements with respect to boundaries are usually made in terms of astronomic latitudes and longitudes. It should here be pointed out that there usually exists a small difference between astronomic and geodetic latitudes or longitudes. For the establishment of control-points for the purpose of map-making, astronomic methods alone have proved unsuitable. In extremely isolated districts precise astronomy is, however, used to some extent for this purpose pending the extension of geodetic triangulation into the area in question.

Geodetic Survey of Canada,
Ottawa, Canada.

PROGRESS IN GEODETIC WORK, APRIL 1929 TO APRIL 1930

William Bowie

The most important event which has occurred in the field of geodesy during the past year was the announcement by President Hoover of his program which calls for the completion of the topographic mapping of the United States in the next eighteen years. The topographic mapping necessarily requires geodetic surveys in the form of triangulation and leveling which furnish the geographic positions and elevations, respectively, on which topographic surveying is based. In order that the geodetic work might be executed in greater amounts than in the past, the President recently authorized an increase in the budget for the geodetic work of the United States Coast and Geodetic Survey. The appropriation bill, covering the funds made available to the Department of Commerce and its bureaus for the fiscal year beginning July 1, 1930, which passed during the present session of Congress, carries $316,624 for geodetic operations. This is a substantial increase over the present appropriation of $88,600.

Expanded program of surveying and mapping--It has been estimated by the Board of Surveys and Maps of the Federal Government and by officials of the Coast and Geodetic Sufvey that the first- and second-order triangulation and leveling of this country can be completed at a cost of between four and five

million dollars. When this work has been completed the arcs of triangulation and lines of leveling will be spaced at intervals of approximately 50 miles, with cross arcs and lines which are needed for the adjustment of the triangulation and leveling nets. The intermediate areas will be covered by third-order triangulation, traverse, and leveling executed just before or during the time when the topographic surveying is being done. It has been generally recognized that the execution of the first- and second-order triangulation and leveling is a federal project, since these classes of work must be carried through the various states, and if the work is done by the individual states, there might be overlapping. Also, they must be executed well ahead of the topographic surveying in order that the maps may be made in any portions of the states in which the topographic mapping is done.

The expanded program for the geodetic surveys calls for the execution of about 2,200 miles of arcs of first-order triangulation during the fiscal year beginning July 1, 1930, in order to make it possible to adjust the triangulation-net of the eastern half of the United States in a way similar to that used in the adjustment of the triangulation-net covering the western half of the country. Some triangulation will be executed in California for the immediate use of the Geological Survey. It is also planned to run about 9,000 miles of first-order leveling during the coming fiscal year.

Other work planned under the expanded program will be the determination of latitudes and longitudes by astronomical observations. The longitude values will be used for determining the deflections of the vertical at triangulation-stations at which astronomical observations for azimuth have been or will be made. The data secured will make it possible to correct the observed azimuths for the deflection of the vertical and the corrected values will then be used in the adjustment of the triangulation. An arc of triangulation tends to swerve from its true direction but is corrected by the use of the astronomical azimuths corrected for deflections of the vertical.

As usual, accurately measured base-lines will be established along the arcs of the triangulation in order to control the lengths as computed through the triangles.

Reopening of Gaithersburg Observatory--The appropriation bill carrying funds for the coming fiscal year authorizes the renewal of observations at the variation of latitude station at Gaithersburg, Md. That station was one of the international group which was operated by the International Geodetic Association from 1900 until 1914. Just after the World War this station, together with certain instruments belonging to it, was transferred to the Coast and Geodetic Survey by the officials of the old Association.

The officials of the Coast and Geodetic Survey and of the Geological Survey will cooperate in making plans for the new program of geodetic work in order that the results may be of immediate value to the latter bureau in carrying on the topographic surveying.

Mississippi River triangulation--During the past year an arc of triangulation was extended along the Mississippi River from the vicinity of Cairo, Illinois, to New Orleans, Louisiana. This arc is one of the principal ones of the country and is very important in the adjustment of the net of the eastern states. Since the results would be of value to the officials of the Corps of Engineers of the United States Army, in connection with flood-control operations, an allotment of $48,000 was made by the Corps to the Coast and Geodetic Survey to cover the increased expense of running the arc along the

River over what the cost of an arc would have been, if located on the higher ground 20 or more miles to the east of the River. This was an important piece of cooperation by two organizations of the government.

Success of Bilby steel towers--On the Mississippi River work a number of troublesome problems were met but they were successfully solved. The Bilby steel tower was used on the triangulation of the river at 210 stations. Owing to the very high trees, extensions had to be added to the tower making the average height 116 feet to the top of the inner tower on which the theodolite was placed and 126 feet to the top of the outer tower used for the signal-lamps and heliotropes. Where towers of this height were not sufficient to raise the lines over the trees, 10-foot extensions were added to the upper part of the tower. The maximum height of tower used was 146 feet to the top of the inner tripod and 156 feet to the top of the outer one.

The towers were carried forward along the upper part of the arc by trucks of two and one-half tons capacity and trailer trucks of two-ton capacity. On the lower part of the arc the highways were not of such a character as to permit the use of the heavy trucks so two additional semi-trailers were purchased and used. There were no cases where the observing party was delayed by lack of towers over the stations where observations were made.

In spite of the very soft ground encountered at some of the stations along the River, it was possible to set anchors in the ground and have the towers so firm and rigid that observations could be made. The average closing-error of the triangles, which is an indication of the accuracy of the work, was 0".89. The specifications for first-order triangulation call for an average triangle closure of about 1".

At intervals of about 15 miles connections were made between the new triangulation and the triangulation-stations or traverse-stations already established along the River for purposes of navigation or flood-control. This will make it possible to compute the geographic positions of these points on the standard datum and make them available for topographic mapping, flood-control surveys, and for the many other uses to which triangulation data are put.

Other arcs of triangulation--An arc of triangulation was executed across New York, Pennsylvania, and into New Jersey. Its terminal points are in the vicinity of Buffalo, New York, and Trenton, New Jersey.

A small amount of first-order triangulation was also executed in the vicinity of Portsmouth, Ohio, which was needed to connect an arc running southward from the Great Lakes through Ohio to the triangulation net in Kentucky.

At the time this report was written a triangulation-party was at work on the arc which extends from Atlanta, Georgia, westward to Shreveport, Louisiana. The work was started at the Atlanta end and has been carried into central Alabama.

Reconnaissance surveys, for the location of arcs of triangulation and the definite selection of the stations at which towers will be erected and observations made, were executed during the year in Iowa, Wisconsin, Illinois, Tennessee, Kentucky, Missouri, Arkansas, Louisiana, Mississippi, Pennsylvania, and New Jersey. The triangulation along some of the arcs involved has already been completed and the work along the other arcs will be done during the next year.

Geodetic work in regions of seismic activity--During theyear triangulation was executed in California for the purpose of determining whether points on the Earth's surface change their geographic positions during or between earthquakes. This work has been carried on for the past six or seven years and has now reached the point where intensive studies are being made acrosš fault zones. The arcs of triangulation being executed are usually comparatively short and the principal stations are connected to existing triangulation. Stations are placed quite close together where an arc of triangulation crosses a fault-zone.

The work in question is done in cooperation with the Advisory Committee in Seismology of the Carnegie Institution of Washington. It is planned to repeat the observations for the angles of the triangles at some time in the future. Whether the interval between observations will be five or ten years is difficult to say.

First-order leveling--First-order levels were run in Pennsylvania and Colorado during the past year. The small amount of leveling was due to the use of most of the funds for arcs of triangulation needed for the eastern adjustment of the triangulation-net. As mentioned above, a greatly expanded program for first-order leveling will be carried out during the next fiscal year.

Field astronomy--One astronomical party made observations for latitude, longitude, and azimuth in the states of Wisconsin, Michigan, Illinois, Ohio, Kentucky, Pennsylvania, and New York. This work was done for the purpose of securing azimuths corrected for the deflection of the vertical to be used in the triangulation-adjustment.

Geodetic work of U. S. Geological Survey--The Geological Survey continued its program toward the completion of the topographic map of the country by executing topographic surveys in a number of states. Much of this work was done in cooperation with the individual states.

Office work--At the office of the Coast and Geodetic Survey in Washington computations and adjustments of the observations made in the field were carried on and the resulting data were prepared for publication. Work was begun on the adjustment of the triangulation-net of the eastern part of the country. In this adjustment will be included part of the triangulation-net of Canada on or near the boundary-line between the United States and Canada. The method employed is that used on the adjustment of the net of the western half of the United States. This method was devised by officials of the Coast and Geodetic Survey and is the only practical one in existence which can be used in the adjustment of a large complicated triangulation-net.

Level-net adjustment--An adjustment was made of the combined level-nets of the United States and Canada. This was necessary because the previous general adjustment, made in 1912, was based on an amount of leveling which was too small and too unevenly distributed to be used in establishing standard elevations for the whole country. It is inevitable that a number of general adjustments for a level-net have to be made because as new lines are added the network is strengthened and more probable values for elevations of bench-marks can be obtained. If the new lines are fitted in to the old ones the closing errors of loops have to be applied only to the new work and corrections greater than are justifiable are required. It is believed that at the present time the leveling net of the country has been so widely extended that the elevations resulting from the adjustment can be held fixed for a long period of time. It is

also believed that they will meet the most exacting requirements of the engineer engaged in surveying and mapping and in construction-operations.

As a preliminary to the final adjustment only one mean sea-level plane was held as zero. This is at the tidal station at Galveston, Texas. The adjustment made with this single tidal plane indicated very clearly that there are differences in the elevation of the tidal planes at the several tidal stations along the Gulf and Atlantic coasts and on the Pacific Coast. The mean sea-levels, determined by tidal observations, increase in elevation with latitude, both for the Atlantic and the Pacific coasts. The Pacific Coast has been found to be higher in elevation than the Atlantic and the average difference is approximately two feet. A table was prepared by which one may obtain the elevations above or below an equipotential surface for the various mean sea-levels at the tidal stations when any one of the tidal planes, other than Galveston, is held fixed.

The geodetic literature of other countries shows that it is not uncommon to have the mean sea-level surface tilt upward with increased latitude. This is true for Russia, France, England, and Japan. No tests have been made in areas lying south of the equator but it is possible that there the mean sea-level surface, as determined by tidal observations at a number of places, would tilt upward as one approached the south pole.

The second step in the adjustment was to hold as zero-elevation mean sea-level as determined at each of the 21 tidal stations along the coasts of the United States and five stations on the coasts of Canada. Necessarily, in obtaining the standard elevations for the interior of the country each of the tidal planes should be given the same weight as each of the others. This adjustment has been made and the results have been sent to the Director of the Geodetic Survey of Canada for his information and for study. The next step will be for the officials of the Coast and Geodetic Survey and of the Geodetic Survey of Canada to confer on the elevations of bench-marks established along the international boundary. It is a question of whether the two organizations can agree on the elevations to be adopted or whether they will find that each country will retain its own elevations. In the latter case, there will be an equation for each of the bench-marks along the boundary which will show the differences in elevations used in surveying, mapping, and other engineering work by the two countries. It is hoped that a decision may be reached in the very near future as to just what elevations will be adopted for the bench-marks along the boundary.

After the boundary-elevations have been agreed on, the final step will be to decide what elevations now in use in the United States can be retained and which should be changed to agree with the elevations obtained by the adjustment of the combined nets of Canada and the United States. Necessarily, the theoretically best elevations would be those resulting from the adjustment. But where the changes are very small, it would seem to be expedient to retain the elevations which have appeared in published or manuscript form.

Adjustment of triangulation in southeast Alaska--Much work has been done at the Washington office of the Coast and Geodetic Survey in computing and adjusting the triangulation of southeastern Alaska. As is well known, an arc of first-order triangulation was extended northward from the vicinity of Point Roberts in the state of Washington, at the western end of the 49th parallel, northward along the coast of British Columbia, and through southeastern Alaska to the head of Lynn Canal, just above Skagway. By means of this triangulation, it is possible to place all the triangulation of southeast Alaska on the

same datum as that used in the United States. A great part of the detailed coast-triangulation of southeastern Alaska has already been fitted into this arc and now the standard geographic positions are available for these triangulation-stations established for the control of the hydrographic surveys and charts.

Publication of results--During the year manuscript and tables have been prepared for the following publications which deal with triangulation and leveling: Special publication No. 156--Triangulation in Hawaii; special publication No. 159--The Bowie method of triangulation-adjustment as applied to the first-order net in the western part of the United States; special publication No. 160--Triangulation in Colorado (1927 datum); special publication No. 164--First-order triangulation in southeast Alaska; special publication No. 161--First-order leveling in Hawaii.

Efforts to improve gravity-apparatus and methods--Studies have been made at the office of the Coast and Geodetic Survey with a view to improving instruments employed on gravity-work. For some years automatic recording of time-signals sent from the Naval Observatory has been employed in the determination of astronomic longitudes and of the rates of chronometers used in gravity-observations. The time-signals have been recorded graphically on a chronograph-sheet where a direct comparison could be made with the beats of the local chronometer. While the method employed has been effective, it was believed that with the great advance in the radio field improvements could be made in apparatus and methods employed. These investigations are still under way and it is hoped that, in the coming six months or a year, decided improvements will be effected.

Progress in gravity and isostasy--Outside of the United States there has been considerable activity in the geodetic field. This is more pronounced in the interpretation of geodetic data than in field work, although the latter has not been neglected. The proof of isostasy by means of geodetic data has increased tremendously the interest of students of the Earth in the forces and processes which have been active in the past and are now in operation to change elevations and geographic positions of points on the Earth's surface. The field of geophysics and of geology will be greatly benefited by the proof of isostasy. The workers in that field now have a very definite basis on which they may build their theories, and their conclusions drawn from geophysical and geological data will be stronger than was previously possible.

The importance of isostasy is indicated by the large number of papers which appear in the scientific journals dealing with the subject and the references to isostasy which are made in books and reports written by geophysicists and geologists.

The most notable work done in geodesy, outside of the United States, is that of Dr. F. A. Vening Meinesz, member of the Dutch Geodetic Commission, in the waters of the East Indies. For a year or more he has been operating on a Dutch submarine and has determined the value of gravity at over 200 points over the waters surrounding the East Indian Archipelago. Only preliminary statements have been made by Dr. Meinesz in letters received by the writer but they indicate that very interesting and valuable data are being collected and that an analysis and discussion of the data will throw much light on the isostatic condition of the Earth's crust under the region where he has worked.

Stockholm meeting--Preparations have been going on during the past year

for the meeting of the Section of Geodesy of the International Geodetic and Geophysical Union which will be held at Stockholm, Sweden, August 1930. Reports will be made at that meeting on the geodetic work accomplished during the past three years in each of about 35 countries which now adhere to the International Geodetic and Geophysical Union. It is expected that the United States will have one or more representatives at the meeting of the Section of Geodesy. These meetings make it possible for geodesists of different countries to become personally acquainted and to carry on cooperative work between triennial conferences. Publications are exchanged and correspondence is carried on. By these means, each country can keep in touch with the work accomplished and the improvements in instruments and methods made in the other countries. It is essential, in the science of geodesy, that the work of the different countries should conform closely to an international standard. Specifications are drawn up and adopted for the several classes of geodetic work and while the international organization has no authority to force any country to adopt these standards, yet the geodesists are more than glad to meet the international requirements and frequently secure even a higher degree of accuracy than is required by the specifications.

U. S. Coast and Geodetic Survey,
Washington, D. C.

THE PRECISION MEASUREMENT OF TIME

Alfred L. Loomis

Due to the unavoidable absence of Dr. Loomis, Prof. E. W. Brown gave an informal account of this work.

THE ABSOLUTE DETERMINATION OF GRAVITY[1)]

Paul R. Heyl

For the absolute determination of gravity there is today no better method than that of the reversible pendulum, the theory of which goes back to Huygens, and the technique of its use to Capt. Kater, over a century ago. The two determinations involved are those of length and time, and it is the determination of length that limits the accuracy of the measurement. Time can be measured to almost any desired precision if one takes time enough about it, but the measurement of length under the best conditions can not be pushed farther than about 0.1μ in a meter, or one part in 10^7. With such conditions as prevail in pendulum-work it is not likely that the precision can be pushed farther than one part in 10^6.

The principal difficulties accompanying the use of the reversible pendulum are found in the numerous corrections that must be applied. Prior to 1884 six such corrections were recognized: The circular correction for amplitude; the effect of change of temperature; the effect of the surrounding air; the effect of lack of perfect sharpness of the knife-edge; the damping effect; and the movement of the support. All these sources of error had been recognized for some time, and the technique of applying the necessary corrections was well understood.

[1)] Publication approved by the Director of the Bureau of Standards of the U. S. Department of Commerce.

In 1884 C. S. Peirce (Report of the Supt. of the U. S. Coast and Geodetic Survey for 1884, Appendix 16) of the U. S. Coast and Geodetic Survey called attention to the existence of an error that had not previously been recognized, the effect of the elasticity of the pendulum. No pendulum can be made perfectly rigid, and every pendulum bends slightly as it swings. The probable magnitude of this correction, as indicated by Peirce's calculations, was disquietingly large. Peirce came to the conclusion that the correction to the length of the pendulum was additive, and equal in amount to the sag exhibited by one end of the pendulum, arranged in a horizontal position with one end firmly clamped. Peirce pointed out that this must be a rather large correction for all pendulums that had ever been made.

Lorensoni (Venezio Instituto Atti, (7)7, 1896, p. 466, amd (7)9, 1898, p.61) gave this matter theoretical attention in 1896 and 1898, but the first really satisfactory treatment of the subject was that of Helmert (Beitrage zur Theorie des Reversionspendels, Potsdam, 1898; Astron. Nach. 143, 1897, p. 349) in 1897. Almansi (Il nuovo Cimento, (4)9, 1898, p. 260; 10, 1899, pp. 85 and 105) published an elaborate theoretical discussion in 1898-99. His correction formula agreed with that of Helmert to the limits of accuracy set by the assumed conditions of the problem.

It might appear at first sight as though this error arose from the fact that the length of the pendulum was shortened slightly by bending, the difference being that between an arc and its chord. I think that this question must have suggested itself more than once in the course of previous work with the reversible pendulum, only to be dismissed with the reflection that such a shortening must be negligibly small, as indeed it is. The difference between an arc one meter long and its chord when the maximum departure of the arc and chord is as much as one mm is only about one part in 50 million.

As a matter of fact, however, the correction for flexibility of the pendulum is additive. In this all theoretical discussions agree and are confirmed by the results of experiment. What takes place is that a part of the gravitative force acting upon the pendulum is spent in bending it. This force is not regained completely when the pendulum straightens out again, but a portion of it is lost as heat. In consequence, the accelerating effect of gravity is diminished and the time of swing lengthened. This effect more than neutralizes the arc-and-chord shortening of the pendulum.

Because of the interest and importance of this newly recognized source of error it was decided by Helmert to undertake a new absolute determination of gravity at the Geodetic Institute at Potsdam. This work was started in 1898, and carried out by Kühnen and Furtwängler. It was finally published in 1906. For the purpose of this determination Helmert obtained the loan of pendulums that had previously been used in absolute determinations of gravity at Vienna and Padua. In addition to the pendulums of the Geodetic Institute, these made a total of five pendulums in all.

Determinations of gravity were made with all these pendulums, paying special attention to the bending-correction. This was calculated by a formula developed by Kühnen and Furtwängler, which was found to give the same numerical result as the formula of Helmert. The correction for the different pendulums was quite considerable, being in some cases as high as 4.7 parts in a million.

The Potsdam determination of the absolute value of gravity is undoubtedly the best that has ever been made. Its weakest point is the fact that the

bending-correction rests upon a purely theoretical formula which was not checked by experiment.

In planning the absolute determination of gravity at the Bureau of Standards it was decided to give some preliminary study to the experimental test of the Helmert bending-formula. For this purpose a pendulum was constructed of a strip of brass 38 mm by 3 mm and 150 cm long. This pendulum was swung from two points of support in succession, first from one end and then from another point 100 cm from the first. In addition, the pendulum was swung in each position in two planes, one parallel to the plane of the strip and one perpendicular to it. In this way it was possible to obtain two pendulums of the same length, differing greatly in flexibility, of the order of 150 to one.

This brass pendulum was swung in a vacuum and corrections made for amplitude and temperature. All other effects except that of flexibility were assumed to be the same in both the rigid and the flexible positions. After making these corrections, there was a difference between the times of swing in the rigid and the flexible positions of about 1,400 parts in a million. By application of the Helmert formula this was reduced to about 300 parts.

Apparently the Helmert formula is only about 80 per cent efficient. On this basis the calculated corrections for the pendulums used at Potsdam are in some cases too low by one or two parts in a million.

In the work now being done at the Bureau of Standards it is planned to place the bending-correction on an experimental basis. Three pendulums of tubular form will probably be constructed, of varying degrees of rigidity, one as high as possible, the others considerably more flexible. By plotting the values of g obtained by means of these three pendulums against their respective flexibilities, a three-point curve will be obtained which will require but a small extrapolation to zero-flexibility.

It is planned to construct these pendulums entirely of fused silica. A standard meter of silica for measurement of their lengths has been constructed.

A Shortt clock has been installed for time-measurements. This is at present in process of being tuned up and rated.

U. S. Bureau of Standards,
Washington, D. C.

THE MEINESZ GRAVITY-APPARATUS ON THE CARNEGIE

Scott E. Forbush and O. W. Torreson

Experience with the Meinesz gravity-apparatus on the gravity-measuring cruise of the U. S. Submarine S-21 during October and November 1928 led to the conclusion that gravity-measurements might be made successfully upon surface vessels and especially upon sailing ships which would be free from engine-vibrations. The survey-vessel Carnegie of the Carnegie Institution of Washington, which had begun Cruise VII in May 1928, seemed to offer an ideal opportunity for experimental work which would test this conclusion, particularly in view of the fact that the vessel's program was one of scientific work exclusively.

It was decided that the installation of a Meinesz gravity-apparatus could

best be made on the Carnegie during her stay in the port of San Francisco in August 1929. However, had it not been for the cooperation of interested persons and organizations, it would not have been possible to do this. Through the efforts of Dr. Meinesz and the kindness of Dr. N. E. Nörlund, Director of the Geodetic Service of Denmark, it was arranged that the apparatus which had been ordered by that Service, and which was nearly completed, be delivered to Washington. A set of invar-pendulums was very kindly loaned by the Netherlands Geodetic Commission, and the complete equipment was standardized at the United States Coast and Geodetic Survey gravity base-station in Washington before being forwarded to San Francisco.

The standardization at Washington and the installation and preliminary records on the Carnegie at San Francisco were made under the supervision of Dr. Fred E. Wright of the Geophysical Laboratory of the Carnegie Institution of Washington, who had been associated with Dr. Meinesz on the cruise of the U.S.S. S-21.

The instrument was placed in the main cabin of the Carnegie, the location being as near to the center of oscillation of the ship as it was possible to determine that point. The plane of suspension of the pendulums lay almost exactly in the plane of the sea-surface.

The installation of apparatus included a special time-signal amplifier which provided for the recording of wireless time-signals photographically upon the trace of the gravity-instrument. However, provision was also made to obtain chronometer-corrections and rates aurally, using either coincident beats of a mean-time chronometer with rhythmic radio time-signals or coincident beats of a fast-running chronometer with mean-time radio signals.

During the preliminary tests at San Francisco it was found that poor radio reception very frequently prevented adequate photographic recording of the time-signals. After leaving San Francisco it was found that the pendulums often could not be swung under the sea-conditions existing during the period of time-signal reception. These factors led to the abandonment of the recording of time-signals and the adoption of the aural method exclusively, and it is estimated that the probable error in determining the chronometer-corrections in that way was generally not greater than ±0.01 second.

The gravity-apparatus on board the Carnegie and the method of observation were essentially the same as described by Dr. Meinesz in his book "Theory and practice of pendulum observations at sea" and more briefly described in an article in Nature of March 23, 1929, concerned with the work on the U.S.S. S-21.

The apparatus consists of a pendulum-apparatus and a recording apparatus, separately encased but combined into one unit and hung in gimbals. The pendulum apparatus contains three half-second pendulums swinging in the same plane. These pendulums are not recorded separately but are combined into two pairs. For each pair the difference of the angle of elongation is recorded, and this angle may be considered as the angle of elongation of a fictitious pendulum of the same period as the original pendulums. It can be shown--and this is the fundamental principle of the method of determining gravity at sea--that the movement of this fictitious pendulum is free from the principal disturbing effect of the ship's movements, that is, the effect of horizontal accelerations. The records of the two pairs which the apparatus provides are therefore nearly regular; there remain only small disturbances by the vertical accelerations of the apparatus and by the tilt of the pendulum swinging-plane. The vertical accelerations are practically eliminated by taking the mean

pendulum-period over a sufficiently long period, and for this purpose one-half hour is more than enough. To apply a correction for the tilt of the swinging-plane the apparatus is provided with an auxiliary pendulum which can move in a plane perpendicular to the swinging-plane and is damped in order to prevent its proper oscillations. The position of this pendulum with regard to the apparatus is recorded.

Besides the records which have been mentioned, two others are made, namely, one of the temperature inside the apparatus and another of the middle pendulum alone. This last record is necessary for computing the reduction of the pendulum-periods to infinitely small amplitudes; it is irregular because the movement of each pendulum is strongly disturbed. Under favorable conditions it is only used for the computation of a small reduction, but, if its amplitude becomes large and the record thereby becomes confused with the fictitious pendulum-records, the accuracy of the observation is greatly increased.

This particular difficulty was experienced in obtaining gravity-records on board the Carnegie on several occasions while she was docked at San Francisco, but it was thought to be due to the effect on the ship of waves set up by the frequent and not far distant ferry-traffic and it was not expected that such irregular movements would be found at sea. This seemed particularly plausible at the time, since a record was obtained on one occasion in the early morning hours after the ferry-traffic had stopped in which the amplitude of the middle-pendulum trace was not unusually large. Subsequent experience at sea indicated, however, that the difficulty was not peculiar to San Francisco Harbor.

The first attempt at gravity-measurement at sea on the Carnegie was made September 6, 1929. Numerous other attempts were made when conditions seemed favorable, that is, when the average roll was about 4° with a maximum of 10° and the average pitch about 2° with a maximum of 3°. On all but one of the occasions the amplitude of the middle-pendulum trace became as much as 100 mm and, moreover, there was definite evidence that one or the other of the outer pendulums had slipped from its initial suspension-position. In the one instance when there was no evidence of either pendulum having slipped, the amplitude of the middle pendulum became 80 mm on the trace.

On one occasion when the Carnegie was drifting very near the West Passage at Penrhyn (or Tongareva) Island two records were obtained in which there was no slipping of the pendulums. On one of these records the amplitude of the middle-pendulum trace reached 80 mm and on the other 45. Judging by the indications of the clinometer, the average roll in these two cases probably was not more than 3° or 4° on either side of the vertical while the maximum probably was not in excess of 7°.

Satisfactory records of gravity were obtained in the ports of San Francisco, Honolulu, and Pago Pago, but only one record among the many attempted at sea seems acceptable for reduction to the desired degree of accuracy. No definite explanation of the unsatisfactory functioning of the apparatus at sea can be given, but experiments were made which revealed some factors which undoubtedly contributed greatly.

The first experiments in an attempt to locate the cause for failure were made in Honolulu Harbor. Here it was found that the whole pendulum-apparatus seemed to have a period of vibration which caused the middle pendulum to vibrate with increasing amplitude. It appeared that the gimbal-stand was not sufficiently rigid, and temporary wooden braces were placed between the stand

and the walls of the cabin which, as far as could be determined in port, would prevent the vibrations which affected the motions of the pendulums. However, subsequent measurements at sea showed no improvement. Permanent bracing was next built on to the gimbal-stand, but this also failed to reduce the large middle-pendulum vibrations.

In order to eliminate, if possible, the slipping of the pendulums upon their supporting planes, various attempts were made to secure the best damping for the oscillations of the apparatus in the gimbal-suspension. The area of the damping-vane was varied and liquids of different viscosities were used for immersion of the vane. The best results were obtained by increasing the area of that part of the vane parallel to the ship's keel 1.5 times and by using heavy automobile oil as the immersing liquid. Even the best arrangement did not, however, eliminate the slipping under sea-conditions.

Should a similar gravity-installation be made again on a sailing ship, particular care should be taken in making the mounting frame and gimbal-stand especially rigid, and the apparatus ought also to be given the greatest possible freedom of movement in the gimbal-supports.

Dr. Meinesz, making gravity-observations on a steamer during a trip from Holland to the Netherlands East Indies in April 1929, encountered difficulties very similar to those encountered on the Carnegie. In a letter concerning his experiences he says: "I made a good number of experiments and several have succeeded although the ship's vibrations caused by the engines (the ship could not stop for the observations) have been disagreeable. The observations which did not succeed all failed for the same reason; the pendulums picked up so much amplitude that I feared slipping of the knive-edges or bumps against the sides so that I had to stop the observation. I could never beforehand say if the amplitude would remain within allowable limits; it often occurred (that) an observation had been satisfactory and that an hour afterwards nothing could be obtained notwithstanding repeated trials and notwithstanding the condition of the sea had not changed. Probably some variation of one of the numerous periods of the ship had brought it to coincidence with the period of the pendulums."

In the light of the experience with the gravity-apparatus on board the Carnegie and the experience of Dr. Meinesz quoted above, it may be seen that the measurement of gravity on surface ships still involves serious difficulties which must be overcome before any great measure of success can be attained.

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GRAVITY-ANOMALIES IN MEXICO

Pedro C. Sanchez

(Translation W. D. Lambert)

By the phrase gravity-anomaly is now meant the difference between the observed value of g at a point on the Earth's surface and the theoretical value Y.

If the observed value is corrected for the elevation of the station above sea-level, for topography, and for isostasy, the anomaly thus obtained is the true one; but if some of the corrections just named are lacking, the anomaly

is merely apparent. If we denote the anomaly by Δg, its value will be $\Delta g = (g - \Upsilon)$. Since the rigorous development of the gravity-potential denoted by W as a function of the latitude and longitude of the place of observation and corresponding to a definite level-surface--which is almost always the geoid--requires an infinite number of terms, which cannot be taken into account, the accepted value of Υ is an approximate theoretical value obtained by dropping spherical harmonics of degree higher than the second or third; moreover, the value obtained for g is to some extent incorrect, for g refers to the point of observation situated in latitude φ and elevation H above sea-level, whereas Υ refers to the corresponding level spheroid defined by the approximate expression for the gravity-potential.

In the case of Mexico the anomalies are the apparent ones, since we have not yet been able to apply the corrections to observed gravity for topography and isostasy, but only for elevation; nevertheless, all things considered, I believe that the discussion of them is of considerable interest for its bearing on the orogeny of our vast territory, as will be seen hereafter.

When gravity is defined in the manner specified, its sign depends on whether g is greater or less than Υ; in the former case the anomaly is positive, in the latter negative.

A plus sign for the anomaly indicates that at the place of observation gravity is in excess of what it would be if isostatic compensation were complete and uniformly distributed to a depth of 113.7 km; a minus sign, on the other hand, indicates that there is a deficiency of gravity in comparison with the theoretical value defined as above.

The correction for elevation to the observed gravity at a given place, called the Faye correction or the free-air correction, as if the point were isolated in space, does not take account either of the topography or of the isostatic compensation but only of the elevation of the station above sea-level.

The correction called Bouguer correction takes into account the elevation of the station, but not the isostatic compensation; it neglects the curvature of the geoid-surface and treats the effect of the topography as if the latter were a horizontal plane of indefinite extent.

These subjects are of current interest and are in the hands of the Committee on Gravity of the Geodetic and Geophysical Union; at the Prague meeting W. Bowie, the eminent American geodesist, was commissioned to prepare a monograph on this subject, in which besides treating of the methods of making gravity-observations he would discuss the formulas for correcting gravity of which we have spoken above and especially the correction that by common accord it was agreed to call the "Bowie correction."

The French geodesist, Defforges, from studies of the gravity-anomalies computed by him reached the following conclusions: (1) Gravity is very unequally distributed over the globe--Clairaut's law, though true on the whole, is affected in almost every case by considerable local anomalies; (2) the shores of the various seas seem to be characterized by slight and almost constant anomalies that vary from one sea to another; (3) islands exhibit considerable excesses of gravity; (4) on the continents there is a deficit of gravity and this deficit usually increases with the elevation of the ground and with its distance from the sea.

Modern observations like those of Nansen on the polar seas and Hecker on

the Atlantic and the Pacific indicate that Defforges' conclusions are subject to numerous exceptions.

Gravity-observations at sea oblige us to assume that the rocks on the bottom are denser, since the mean density of sea-water at several kilometers depth is 1.03, much less than the density of the rock of the Earth's crust, which varies between 2.2 and 2.8; and if there were no excess of density to compensate the small density of the sea, gravity would be decidedly less than on the continents. Since this is not the case, but since on the contrary observation has shown that gravity is sensibly equal to that on the continents, it is therefore necessary to accept a greater density for the rocks of the ocean-bottom.

Observations in the Atlantic, Indian, and Pacific oceans have shown that gravity has the same value in deep and wide seas in which the depth is uniform as over the continental plains, which is a strong argument in favor of isostasy in the Earth's crust. The greater volume of the continental masses is presumably compensated by the smaller density of the matter composing it, and in the oceanic portions the smaller density of the sea-water is compensated by the greater density of the strata forming the ocean-floor.

On examining the great mass of data afforded by modern observation the conclusion is that the anomalies are not accidental but have a well-defined systematic character, since there exist some regions where positive anomalies predominate and others where anomalies are negative. Generally speaking it may be said that positive anomalies prevail on oceanic islands, whereas among the mountains negative anomalies are frequent.

When hypotheses--more or less plausible--are made regarding the position and size of the subterranean deficiency of matter and when the thickness of the compensating layer is spoken of, this can not be done without specifying the amount of the anomaly and representing it in a clear and comprehensive manner, although unfortunately this may not correspond to the actual distribution of subterranean matter in the crust.

It is known that the attraction of a stratum of rock of indefinite extent upon a point on its upper surface is $(3g/2)(h/R)(\theta/\theta_m)$, where h and θ are the thickness and density of the stratum, g the intensity of gravity, and R and θ_m the radius and mean density of the Earth. If we assume in round numbers g = 978 cm, R = 6,370,000 meters, and $\theta/\theta_m = 1/2$, the preceding formula becomes $0.00011515\ h$, the attraction of the layer being in centimeters and h in meters.

For example, at Sandakphu (Himalaya) the anomaly is -0.150 cm; therefore the thickness of the cap whose attraction is equal to this would be -0.150/0.00011515 = 1,303 meters; and since the elevation of the station above sea-level is 3,589 meters, it results that the visible masses are only one-third compensated for the ideal layer considered.

When the anomalies over wide regions of the globe are studied it appears that they oscillate about a mean value that for some regions is positive and for others negative.

In small regions the results show clearly the influence of the topographic forms, as is shown at stations situated at a considerable elevation above the average level of the region considered, especially mountain tops, which give very large anomalies.

A study of the isanomalous lines in central Europe and southern Japan shows

that these lines follow the relief rather closely; that the zones of negative anomalies correspond to mountainous continental regions and those of positive anomaly to almost level-regions or those close to the sea.

The absolute maximum values of negative anomaly are not exactly on the axis of the mountain chain but along a line parallel to it and opposite to the greater depression nearby. The maximum values of positive anomalies are not in the depressions but near the places in which the anomalies are negative. Costanzi's map for Italy shows that the positive anomalies, instead of being found over the Ligurian and Tyrrhenian seas, penetrate into the continent, invade part of the Maritime and Ligurian Alps, and even the Apennines and the north Sicilian range.

In the Tyrrhenian regions of Calabria and Sicily, which face the great marine deeps, the isanomalous lines are parallel to the coast; whereas in the southern part of Sicily these curves are normal to the coast and are elongated towards the shoals in the direction of Malta. The maximum observed positive anomaly (+167) is on the volcanic island of Stromboli and the maximum negative (-70) in the center of Sicily.

In the regions of greater seismic activity, western Calabria, the Lipari Islands, northern and eastern Sicily, the lines of equal anomaly are very irregular, twisted, and more closely spaced than anywhere else, a condition that indicates a rapid and irregular variation of the underlying masses and consequently instability of the ground.

A study of the lines of equal anomaly in Spain suggests a relation between the topography and the anomalies, which tend to be positive on the coast and negative toward the interior, while the greater the anomaly the greater the elevation of the region considered.

It may be said that in a general way positive anomalies prevail over the greater part of Europe except Sweden, Norway, and Finland, where negative anomalies predominate. In Spain, southern France, northern Italy, Dalmatia, and the Alpine area the anomalies are variable, sometimes positive, sometimes negative, no prevailing tendency being noted.

In central Asia conditions are opposite to those in Europe, for there is found a vast region of negative anomaly, especially in the upper reaches of the Amu Darya and the Syr Darya. In the northern part of the Red Sea negative anomalies are found. In western Turkestan and in British India there is no prevailing tendency as to the sign of the anomaly.

In the United States where the Bouguer or the Faye correction is applied the anomalies show a close relation to the topography and the elevation above sea-level. It is noticed also that in regions where pre-Cambrian or Mesozoic rock prevails the anomalies are positive; in Palaeozoic, Tertiary, and Quarternary regions the anomalies are negative.

Let us now see what is shown by the gravity-anomalies in our territories. In Figure 1 are shown the gravity-stations, as yet few in number, but enough, nevertheless, to allow lines of equal anomaly to be drawn. All numerical data are given in Table 1. For simplicity we have taken the thousandth of a dyne for the unit; the numbers inscribed on the curves are likewise thousandths of a dyne.

If we consider merely the absolute values, there is nothing in them to

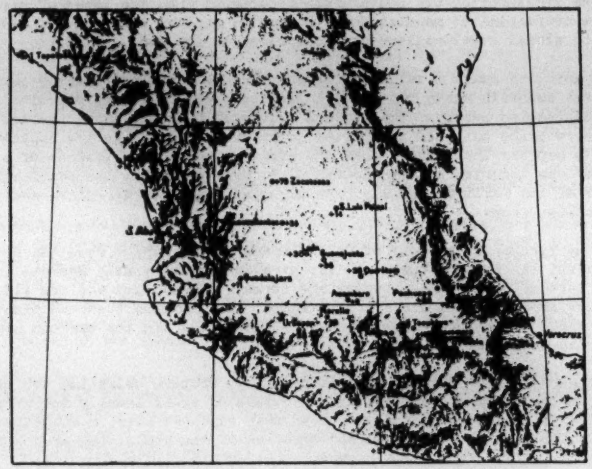

Fig. 1--Gravity-stations in the Republic of Mexico (figures indicate gravity-anomalies expressed in thousandths of a dyne)

attract attention, since similar values are found in any part of the world.

Figure 2 shows clearly the influence of the elevation of the gravity-station. It is drawn by taking the latitudes as abscissas and the elevation above sea-level as ordinates.

Considering the fact that Orizaba is 1,200 meters above sea-level, the anomaly is not very large, and I attribute it to the influence of the volcanic axis, since, as may be seen in Figure 3, the line of zero anomaly follows this axis, near which Orizaba is situated.

In the region of Jalapa the lines of anomaly +40 and +60 show clearly the influence of the Cofre de Perote, an important volcano the eruptions of which cover a very extensive region. The inflexion of the lines of equal anomaly at Tacubaya and Toluca indicate the predominance and the considerable effect of basalt-flows.

In Figure 3 the lines of equal anomaly are drawn on an orohydrographic map in order better to show the influence of the mountain system.

The position of the line of zero-anomaly is noteworthy in that it conforms closely to the volcanic axis. This axis is the most important in the Republic, since along it are ranged our principal volcanoes: San Martin Tuxtla, Pico de Orizaba, Popocatepetl, Ajusco, Nevado de Toluca, Tancitaro, Jorullo, and Nevado de Colima. To the south of it lie all the earthquake-epicenters of the country spreading out towards the interior of our territory,

Table 1--Gravity anomalies at Mexican stations

[The value of Y is computed by the Bowie formula of 1917: $Y = 978.039\ (1 + 0.005294 \sin^2 \varphi - 0.000007 \sin^2 2\varphi)$. The correction as in free-air is computed by the formula: $c = 0.0003086H$ where H = elevation in meters. In the table the free-air correction for elevation is applied to the theoretical gravity at sea-level instead of to the observed gravity as in the text of the article--the sign of the correction is therefore reversed.]

Circuit	Station	Latitude	Longitude	Altitude H	Year	Observer	Observed g	Free-air corr'n	Calculated Y	Reduced g	Anomaly (g-Y)	Probable error
		° '	° '	meters			dynes	dyne	dynes	dynes	dyne	dyne
I	Tacubaya, Federal District	19 24.3	99 11.7	2,299	1912	A. Leyva	977.936	-0.709	978.608	977.899	+0.037	-------
II	Puebla, Puebla	19 02.7	98 12.7	2,153	1925	Bracho	977.992	-0.664	978.567	977.923	+0.069	±0.0007
	Tehuacan, Puebla	18 28.5	97 23.6	1,614	1925	Bracho	978.079	-0.498	978.557	978.059	+0.020	±0.0006
	Orizava, Vera Cruz	18 50.9	97 06.0	1,241	1925	Bracho	978.201	-0.383	978.557	978.194	+0.007	±0.0004
	Jalapa, Vera Cruz	19 32.0	96 55.0	1,415	1925	Bracho	978.243	-0.437	978.615	978.178	+0.065	±0.0002
	Vera Cruz, Vera Cruz	19 12.0	96 07.9	4	1925	Bracho	978.587	-0.001	978.596	978.595	-0.008	±0.0002
III	Durango, Durango	24 01.7	104 40.2	1,886	1925	Bracho	978.318	-0.582	978.894	978.312	+0.006	±0.0001
IV	Colima, Colima	19 14.6	103 43.8	456	1926	Bracho	978.426	-0.141	978.599	978.458	-0.032	±0.0004
	Zacatecas, Zacatecas	22 46.7	102 34.0	2,596	1926	Bracho	978.089	-0.801	978.810	978.011	+0.079	±0.001
	Pachuca, Hidalgo	20 07.7	98 43.9	2,425	1927	Bracho	977.990	-0.748	978.650	977.902	+0.088	±0.0015
	Leon, Guanajuato	21 07.5	101 40.9	1,805	1927	Bracho	978.162	-0.557	978.709	978.152	+0.030	±0.0003
	Queretaro, Queretaro	20 35.6	100 23.9	1,820	1927	Bracho	978.140	-0.562	978.676	978.114	+0.026	±0.0005
	Aguas Calientes, Aguas Calientes	21 52.8	100 18.0	1,869	1927	M. Medina	978.202	-0.577	978.755	978.178	+0.024	±0.001
	San Luis Potosi, San Luis Potosi	22 09.2	101 05.0	1,865	1927	M. Medina	978.210	-0.576	978.772	978.196	+0.014	±0.005
	Guanajuato, Guanajuato	21 01.0	101 15.3	2,033	1927	M. Medina	978.111	-0.627	978.702	978.075	+0.036	±0.037
V	Iguala, Guerrero	18 21.1	99 32.5	731	1927	E. Ruiz	978.262	-0.226	978.550	978.324	-0.062	±0.002
	Acapulco, Guerrero	16 50.4	99 54.7	2	1927	E. Ruiz	978.527	-0.001	978.471	978.470	+0.057	±0.001
VI	Purisima Concepcion Mine:											
	North shaft	20 08.4	98 40.3	2,865	1928	E. Ruiz	977.906	-0.884	978.650	977.766	+0.140	±0.001
	500-meter level	20 08.4	98 40.3	2,264	1928	E. Ruiz	977.984	-0.699	978.650	977.951	+0.033	±0.001
	Tulancingo, Hidalgo	20 05.0	98 22.0	2,155	1928	E. Ruiz	978.034	-0.665	978.647	977.982	+0.052	±0.0004
VII	Topolobampo, Sinaloa	25 36.0	109 03.0	11	1928	E. Ruiz	978.999	-0.003	979.001	978.998	+0.001	±0.001
	Mazatlan, Sinaloa	23 11.3	106 25.7	5	1928	E. Ruiz	978.850	-0.002	978.838	978.836	+0.014	±0.0004
	Tepic, Nayarit	21 30.6	104 53.9	925	1928	E. Ruiz	978.469	-0.285	978.732	978.447	+0.022	±0.0006
	San Blas, Nayarit	21 32.5	105 17.3	2	1928	E. Ruiz	978.710	-0.001	978.733	978.732	-0.022	±0.0005
	Guadalajara, Jalisco	20 40.8	103 20.5	1,564	1928	E. Ruiz	978.203	-0.489	978.681	978.192	+0.011	±0.001
VIII	Tlamacas, Puebla	19 04.0	98 37.5	3,885	1929	E. Ruiz	977.577	-1.199	978.589	977.390	+0.187	±0.005
	Amecameca, Mexico	19 07.6	98 46.0	2,468	1929	E. Ruiz	977.900	-0.762	978.593	977.831	+0.069	±0.0008
IX	Cuernavaca, Morelos	18 54.9	99 14.2	1,545	1929	E. Ruiz	978.106	-0.477	978.581	978.104	+0.002	±0.001
	Toluca, Mexico	19 17.5	99 39.6	2,664	1929	E. Ruiz	977.852	-0.822	978.601	977.779	+0.073	±0.001
	Uruapam, Michoacan	19 25.0	101 57.3	1,631	1929	E. Ruiz	978.153	-0.503	978.608	978.105	+0.028	±0.0008
	Morelia, Michoacan	19 42.0	101 07.2	1,903	1929	E. Ruiz	978.066	-0.587	978.625	978.038	+0.028	±0.0003
	Acambaro, Guanajuato	20 02.0	100 43.4	1,856	1929	E. Ruiz	978.061	-0.573	978.644	978.071	+0.010	±0.0006

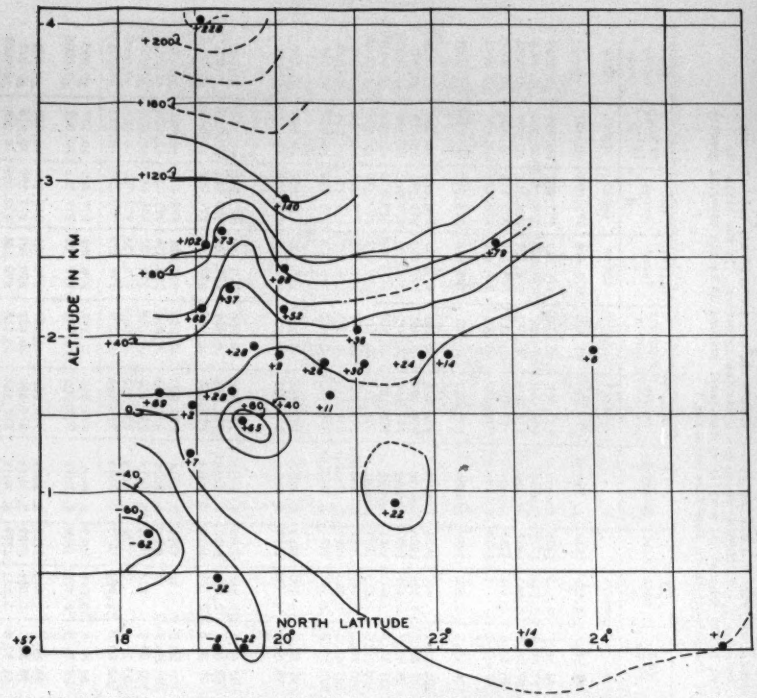

Fig. 2--Gravity-anomalies in the Republic of Mexico with relation to altitude and latitude

whereas to the north seismic stability is nearly complete.

If we consider the direction of the mountain-ranges that surround our central plateau, it is seen that to the north of the volcanic axis the northwest-southeast direction prevails and to the south the east-west, which is the characteristic thrust-line of the Antilles. Moreover, when the sequence of eruptions is studied it appears that these have taken place from east to west in such a way that the volcano nearest to the Pacific is always the most recent.

The opinion of geologists is that we are dealing with a rift, transverse as regards the Republic but not as regards the terrestrial globe, which doubtless corresponds to a great fault coming from the inner Antilles and prolonged into the Pacific. Negative lines of equal anomaly extend toward the coast but probably do not reach it, since at Acapulco a positive anomaly appears. Lines

Fig. 3--Curves of equal gravity-anomaly in Mexico

of equal positive anomaly extend over the Central Plateau increasing in value with the elevation and perhaps with the thickness of the effusive rocks.

I do not know now whether lines of equal positive anomaly reach as far as the Eastern Sierra Madre, which belong entirely to the Cretaceous, a condition which would be by no means unusual, since positive anomalies prevail in the Mezozoic; and if this is the case it would still further emphasize the effect of the volcanic axis in separating two very distinct regions, the unstable to the south and the stable to the north of this axis, which, as I have elsewhere shown, is an extremely important topographic feature and must moreover be considered as the natural boundary of North America.

The anomalies are extremely interesting since by comparison with a geological chart it is seen that the positive anomalies occupy almost entirely the region of effusive rocks, whereas negative anomalies prevail in the region of the Archean masses forming the horsts (pillars) that support the Cretaceous folds.

As soon as possible I expect to continue the gravity-observations, since from the summary investigation that I have made, the importance of the subject is evident. The conclusion to which the discussion has led me tends to confirm those reached in my work entitled "Estudio orogenico de la Republica Mexicana."

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THE STUDY OF EARTH-TIDES FROM THE VARIATION IN LATITUDE

Harlan T. Stetson

Various experiments have been made at various times and places for detecting the theoretical tide in the Earth's crust caused by the gravitation-action of the moon. The most successful of these, perhaps, has been the Michelson-Gale experiment at the Yerkes Observatory, Williams Bay, Wisconsin. Here the tilt-method was employed, and the water tides observed in a pipe 500 feet long gave substantially the results predicted on the presumption of a solid Earth of coefficient of rigidity 0.6.

Realizing the high accuracy attainable in the determination of the position of the zenith by astronomical methods with the photographic zenith-tube, the writer undertook the search for a lunar correlation with latitude-variation in the early part of 1928. With the assistance of Miss Margaret Olmsted, analysis was made of the observations for latitude obtained at Gaithersburg, Maryland, by Dr. Ross in 1911-14. The original observations were published in the publications of the United States Coast and Geodetic Survey and were a part of the series undertaken by the International Latitude Survey inaugurated by the International Astronomical Union.

Preliminary results of the present investigation were first announced at the meeting of the American Astronomical Society at Amherst in August 1928, and indicated a marked correlation of latitude-variation with the position of the moon in the sky of the observer at the time such observations were made.

In the analysis it is to be noted that the observations were restricted to the results obtained with the photographic zenith-telescope, thus eliminating the observer's personal equation.

For convenience the observations were divided into two periods, one from 1911 to 1913, the other from 1913 to 1914. According to Ross's estimates, the 1913 to 1914 observations were considerably superior to those of the earlier years, as is evidenced by the smaller probable error.

In conducting the analysis a card-catalog was made of the results of the observations of latitude for each night and each group of stars. The mean right-ascension of the groups gave the necessary data for ascertaining the moon's hour-angle at the time of observation. From the mean curve of latitude-variation at Gaithersburg, extending over the period 1911 to 1914 and published by Ross, corrections were obtained to reduce each night's data to the mean latitude of Gaithersburg determined from the observations of the whole period. The resultant values of latitude were then tabulated against the mean value of the moon's hour-angle for each group of stars, and the running mean, taking three at a time, gave the results graphically shown in Figure 1.

Since the declination of the moon, and hence the observer's distance from the sub-lunar point, may vary greatly even for the same values of the hour-angles, the hour-angles and declinations were transformed into altitudes and bearings by suitable tables. Again, the observations were divided into two series, one including those made when the moon was above the horizon, and the other when it was below the horizon.

The striking rise in the value of the latitude with the increasing altitude of the moon is shown strikingly in the altitude-latitude curve, Figure 2, which again was plotted from the running means. The maximum latitude occurs

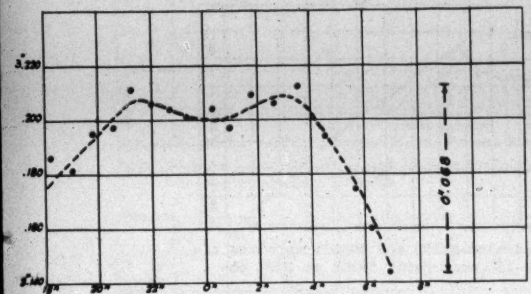

Fig. 1--Latitude-variation and moon's hour-angle, 1913-14 (each point based on 300± observations)

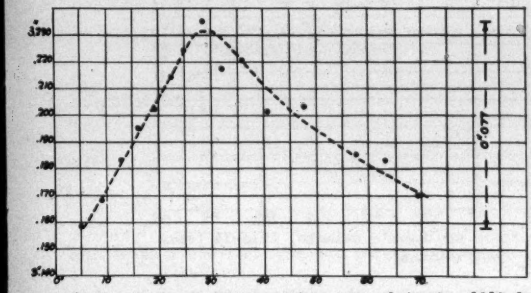

Fig. 2--Latitude-variation and moon's altitude, 1913-14 (each point based on 300± observations)

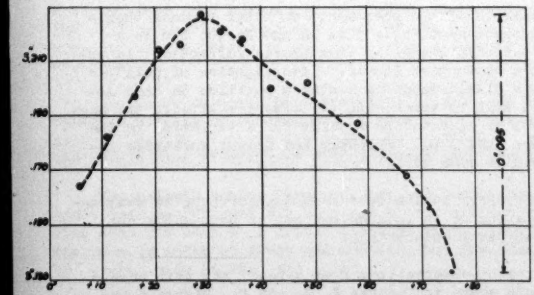

Fig. 3--Latitude-variation and moon's altitude, all observations, 1911-14 (each point marked by circle based on 600± observations, one point marked by dot on 300± and other on 74±)

at altitude 30°, or when the observer was 60° from the sub-lunar point. It should be stated that the extreme range of variation of latitude due to this lunar effect was 0".08 for the 1913-14 series and about 0".09 for the whole series, 1911-14. On account of the relatively greater degree of precision obtained in the 1913-14 series and the larger number of observations included, double weight was given this series in plotting the final graph as exhibited in Figure 3. The fact that the total variation is about 20 times the probable error for each point on the curve leaves little ground for interpreting the curve as a chance phenomenon. The curve of observations for the moon below the horizon is radically different, as is shown in Figure 4. A marked fall in the value of latitude follows the negative altitude of 30°.

The change in latitude as a function of the moon's azimuth is shown in Figure 5.

This exhibition of a variation in latitude with the rising and setting of the moon expresses graphically a shift in the direction of the vertical, which in the most liberal meaning of the word is essentially tidal.

In seeking an explanation for this

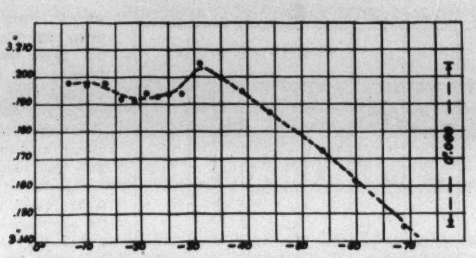

Fig. 4--Latitude-variation and moon's negative altitude, 1913-14 (each point based on 300± observations)

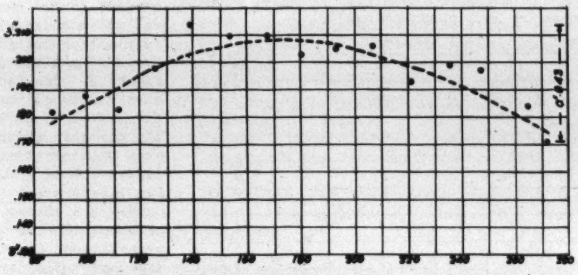

Fig. 5--Latitude-variation and moon's azimuth, 1913-14 (each point based on 300± observations)

extraordinary relation, one is embarrassed by the magnitude of the range of fluctuation, which is five to ten times the amount expected from the theoretically derived earth-tide.

Possibilities for explanation of the phenomenon on other grounds than a deflection of the vertical brought about by a tide in the Earth, led to a consideration of a possible periodic change in atmospheric refraction, introduced by a tide in the Earth's atmosphere itself. Investigation of this led to the conclusion that while a displacement of a star's position on this account would be of the correct sign to agree with the observed effect, the magnitude of the effect could not be expected to account for a variation in the position of the zenith by more than 0".01. Probably the actual variation introduced on this account would be even less.

The removal of the photographic zenith-tube from Gaithersburg to Washington made possible the accumulation of an independent set of data under the direction of the United States Naval Observatory.

An analysis of the Washington observations from 1924-27 was next undertaken, the results of which are shown in Figures 6, 7, and 8. Figure 6 unmistakably shows the same variation dependent upon the moon's hour-angle and declination which was shown in the Gaithersburg series, but with some out-

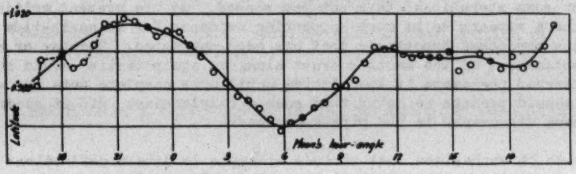

Fig. 6--Latitude-variation and moon's hour-angle, Washington observations, 1924-27

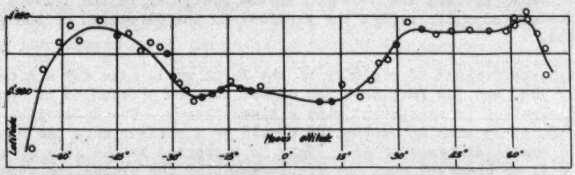

Fig. 7--Latitude-variation and moon's altitude, Washington observations, 1924-27

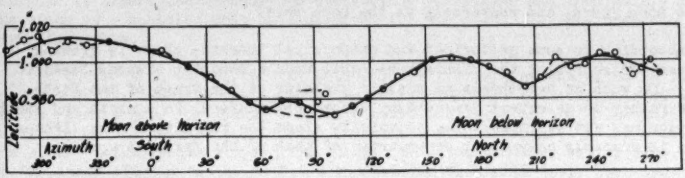

Fig. 8--Latitude-variation and moon's azimuth, Washington observations, 1924-27

standing differences.

The Washington observations, however, give more nearly a sine-curve. When examined from hour-angle 18 a sharp minimum occurs near hour-angle 6. The extraordinarily large amplitude of the curve as compared with the probable error of single points on the curve give assurance of the reality of the correlation.

The curve showing the correlation of altitude with latitude, Figure 7, exhibits a range of about 0".04, or about half that shown by the Gaithersburg series. Maxima occur at altitudes -45° and +40°. Another maxima at 65° is the outstanding discrepancy when compared with the Gaithersburg series.

The azimuth curve, Figure 8, for the Washington observations follows again very approximately a sine-curve, the maximum values for latitude occurring when the moon is a little north of west. A secondary maximum occurs when the moon is north at lower culmination, but two secondary minima occur, each 30° either side of the east point.

Further researches may reveal among the possible causes of this latitude-variation a change in the direction of the Earth's instantaneous axis of

rotation or some aberational term not yet sensed. At the present writing, however, there appears to be more supporting evidence for an earth-tide theory than perhaps any other hypothesis that has been suggested. Whether or not an actual displacement of the Earth's crust along an equipotential could account for the observed phenomena is speculative until more complete data are available. It should perhaps be noted that such a displacement, did it occur, could not have been discovered in the pipe-experiment.

It is to be emphasized that various attempts to detect deflections in the direction of gravity by the plumb-line, horizontal pendulum, or a pipe-experiment such as that of Michelson and Gale, refer all changes to the positions of the Earth's crust, whereas the location of the zenith as in the Talcott method for latitude-determination refers the vertical to the direction of the Earth's axis in space.

The high coefficient of rigidity of the Earth would make for a tide with small if any lag, and the fact that the effect is more pronounced at upper than at lower culmination is consistent with a tidal theory. The deflection of the vertical produced by such an earth-tide should be a maximum when the moon culminates at a zenith-distance of 45°. This is supported by the form of the curve. It will be observed that the displacement of the plumb-line under these conditions is such as to increase the declination of the zenith at the upper culmination of the moon, which means an increase in the value of the latitude as the moon rises, and conversely as the moon sets.

According to some geological and geophysical theories there is growing evidence for supposing the lithosphere rests upon a somewhat viscous basaltic layer. On such an hypothesis an elastic yielding of the crust of the Earth, to the rather large extent demanded by these observations, in a north and south direction may not be impossible, especially since the rigidity of the lithosphere is probably only about one-quarter of that of the Earth as a whole.

A displacement of the zenith should occur in an east-and-west, as well as a north-and-south, direction. If our theory is correct, the east-and-west displacement would have its maximum when the moon is some 55° from the meridian. An analysis of clock-rates and the variation in longitude should give some indication of this effect in the east-and-west direction.

Whatever may be the causes involved, the importance of the consequences of such an observed effect scarcely needs to be emphasized, as it vitally concerns: (1) The fundamental determination of star-positions; (2) the fixing of any fundamental point on the Earth; (3) geological and geophysical theories of the structure of the Earth itself.

A study of the Ukiah observations is now being made at the Perkins Observatory. For the continuance of this investigation acknowledgment is due the National Academy for a grant from the Gould Fund.

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SECTION OF SEISMOLOGY

(Washington, D. C., joint meetings with Eastern Section of Seismological Society of America, May 5 and 6, 1930[1])

THE EARTHQUAKE, A JOINT PROBLEM OF THE SEISMOLOGIST AND ENGINEER

N. H. Heck

In every branch of science there comes a time when a review of progress and an attempt to outline future plans for development are of equal importance to the work being done at the time. We have in my opinion reached such a time in seismology in the United States. There can be no difference of opinion in regard to the importance of the investigation of the central region of an earthquake to both the seismologist and to the engineer. Since earthquakes occur with relative infrequency, there is also need for simulating earthquakes in order to study earthquake-effects on structures. The viewpoint of the seismologist and that of the engineer interested in earthquakes are not necessarily the same. While deploring the destruction, the seismologist finds the most destructive earthquake as well as one which occurred in a region where it can do no harm as of equal value in furnishing useful information about the transmission of earthquake-waves and the condition of the interior of the Earth, while the engineer is interested in only the destructive earthquake and then mainly in the study of structures that have survived for the useful lessons that can be drawn from them.

An interesting example of the seismologist's viewpoint was given in Tokyo on September 1, 1923, though I cannot guarantee that all seismologists will act in the same manner. Dr. Imamura and his colleagues were at the Seismological Institute of the University of Tokyo when the earthquake came. At once all but one of the seismographs stopped recording. Soon the building was on fire. Part of the staff climbed to the roof and put out the fire with their bare hands while the rest put into operation such instruments as were capable of it. They, of course, knew nothing of what was happening to their families and homes.

It is my purpose to discuss the interrelations of the work of the seismologist and engineer. While the aims are different their work can scarcely be separated. This applies not only to the work in the region of damage but to the teleseismic field. To have them work entirely independently would be disastrous and for the results desired by the engineer fatal. There are relations which do not even appear at all obvious. I had occasion some time ago to bring together a prominent seismologist and an engineer who has done outstanding work in strength of materials, especially in the field of fatigue. They soon found a common field and engaged in a valuable interchange of views.

The engineer wants to know just what goes on in the central region of a severe earthquake. The seismologist can not tell him with any degree of completeness since the only continuous record of this sort ever made was at Tokyo in 1923. There was but a single instrument and, of course, at only one point

[1]Only those papers contributed by members of the American Geophysical Union at the joint meetings are included in these transactions; the first two papers here published were presented May 5 and the others May 6, 1930. It is expected that all of the papers presented at the joint meetings, as well as the symposium led by H. F. Reid on "The publication of earthquake data," will be published by the Eastern Section in a special bulletin.

and the engineer is not only not satisfied with such limited data but he also wants to know the difference in earthquake-effects with various subsurface-conditions. The seismologist generally places his instruments on rock with the purpose of reducing the amplitudes while the engineer wants to know how great the amplitudes can be in alluvial and soft material.

Much attention has been given to devices consisting of a series of small columns each of which falls when the acceleration attains a certain amount. The purpose is to find the maximum acceleration at the place of observation. This is too simple a solution of a complex problem and not only is the information limited to a single effect, the maximum acceleration, but even this is not reliable because the instrument does not take care of certain factors. The need is not infrequently expressed for a cheap instrument which can be placed in large numbers throughout the central region of a possible earthquake. I held this view until recently, but I have now definitely changed it. I believe that far more can be learned from a smaller number of instruments which, while able to record the full strength of the earthquake and to survive violent treatment, are yet real seismographs. In order that we may arrive at useful results, earth-tilt and resonance-effects must be eliminated from the records. Tilt will make impossible the interpretation of the record, though I think that the tilt itself should be measured by other means; also damping is necessary. With these things accomplished the information desired by the engineer would be forthcoming. At the same time I believe that information can be obtained without extra cost which is greatly needed by the seismologist--how the earthquake-waves are propagated from the epicentral region to the place where they begin to have the regular features of the teleseismic records. This requires accurate time but it is to be hoped that within a few years there will be broadcasting of time, either continuously or much more frequently than at present. It is proposed that the instruments referred to be set in motion by the earthquake itself, it being understood that they will be placed in a region where more delicate instruments will record the first severe shock. This is not a dream of the future inasmuch as a modification of an existing instrument will apparently accomplish the purpose. This will be described in another paper to be presented at this meeting.

It might be objected that we might place a number of instruments in a region and then have to wait years for results. This might be true if we insisted in waiting for the major destructive earthquakes which fortunately occur at very long intervals. However, this is totally unnecessary. There are parts of the country where earthquakes of force 7 occur not infrequently and the chance of one occurring within a few years is quite strong. By setting the instrument so that such a shock would start it we would begin to get information very soon after the installation was completed. An example of a possible region for such an investigation is the Imperial Valley in California and here there appear to be earthquakes of considerable intensity every few years. The only objection to this region is the difficulty in finding suitable sites on account of the desert character of much of the region immediately surrounding the irrigated portions of the valley.

While the determination of exact facts within the central region of an earthquake is of vital importance, there is no reason to wait for this information before undertaking the effects on structures. Shaking-platform tests have great possibilities and we already know enough about earthquakes to be able to operate them somewhat in the manner of an earthquake. A more serious problem is the necessity of using models of full-size structures for any platform that can be conceived of as practicable. This question must receive much more study before we can be sure of the relation of observed effects on models and those

on full-size structures. Dr. Tuckerman of the Bureau of Standards has expressed the view that this is one of the most important phases of the problem from the viewpoint of the engineer.

The problem of recording adequately what goes on in the central region is by no means simple. This is evidenced by inspection of the fall of monuments in cemeteries. The case of Attica, New York, was particularly instructive. In a small cemetery east of the town every monument went down. The vast majority fell in a southerly direction and a few northerly. Two were at quite a different angle and one at right-angles to these directions. At another cemetery no monuments went down but several were twisted on their bases and by approximately the same amount.

It seems probable that these are not thrown down by a single impulse but by accumulation, possibly with resonance effects. It seems almost certain that this is true of chimneys. An interesting illustration of this occurred at Attica. In one section of the town practically every chimney was down, some of them of the best modern construction. Right in the midst of them there was an old hotel so old that it was a stagehouse in the days before railroads existed in the region. The chimneys were nearly ready to fall so that a wire guy has been attached to prevent this. These chimneys stood though there was evidence of disturbance at the base.

These examples are intended to strengthen my argument for complete records of the disturbance in the central region.

Engineers naturally feel that teleseismic study has made few contributions to the needs of the engineer. The contribution is not direct, but the indirect contribution, and especially that which can be made in the future, is by no means small. I feel very certain that when we have set up instruments such as I have described and have recorded the complicated series of waves in the central region of an earthquake, it is going to be rather hard to put the information in such form that it can be used by the engineer. The best hope of accomplishing this lies in the fact that seismologists have learned to handle the records obtained at some distance and reduce them to an orderly system. The correct plan, therefore, seems to be to work from the comparatively simple conditions outside the epicentral region towards the center. Even with this aid it is not going to be easy to find a satisfactory solution of all the problems.

Again in the development of instruments, I have stated that an instrument is now available or can readily be made so which will give the facts desired by the engineer. This is a modification of a teleseismic instrument, and it is the only way in which an adequate instrument could be developed.

Neither the seismologist nor the engineer can afford to overlook the importance of geological questions in this connection. Questions have come up recently as to the best alignment of a long factory-building in an earthquake-region so that certain delicate operations are least likely to be affected. The determination of this fact is not easy from surface-evidence alone. In some cases teleseismic evidence from more than 1,000 miles away has shown that the direction of the first slip is at right-angles to that which might have been inferred from the geological studies of the surface-conditions.

Another important question is whether the type of investigation being carried on at the present time in southern California has any elements which will serve to meet the needs of the engineer inasmuch as the instruments will go out of commission as soon as a severe earthquake begins. My answer to

this is that no one is more concerned than the engineer as to the possibilities of an earthquake in a given region. Very often engineering design takes into account probable conditions and rarely is it found feasible to design for the maximum conditions. The one hope of finding out what is going on in a region lies in the plan that is being followed in California of accurately recording the conditions along a known fault-line. Sooner or later the significance of a particular type of earthquake-activity will be known and there may even be a possibility of prediction in a general way. The precise history of the earthquake-activity of a region over a period of years is going to be of great value. Incidentally I think that the problem here undertaken should be considered from the engineering viewpoint. The conditions in the Earth when the stress is developing, which results from an earthquake, is somewhat analogous to the testing of materials in a giant testing-machine.

Plans are under way for the installation of seismograph-stations in the Mississippi Valley region so that we may expect that before long there will be instruments in St. Louis (Missouri), Little Rock (Arkansas), and in western Kentucky, the investigation being carried on under the auspices of the St. Louis University and the National Research Council. Here the conditions are somewhat different from California. If an earthquake of great intensity occurs, it is likely to affect a very large area and this type of earthquake presents a more difficult field of investigation than the type which occurs in California. I feel that for the present the investigation in this region should be confined to that now contemplated, and that engineering studies should not as yet be undertaken as studies in California are far more likely to be productive of valuable results.

None will deny that the question whether another great earthquake will occur in the Mississippi Valley, knowing that a very great one occurred in 1811 and another about a century earlier, if surface-evidence and Indian tradition may be accepted. There is only one way in which this can be determined; that is, by studies with precise instruments. Instruments which record what happens during a great earthquake may not be called upon to furnish a record for a century, yet the problem from an engineering and insurance viewpoint is one of considerable importance.

Very often it is not possible to deduce the subsurface-conditions and their probable effect on earthquake-damage from inspection so the earthquake itself must be used. Naturally we can not wait for the severe earthquake to come along and it, therefore, is an important question whether any useful information can be deduced from the effects of moderate earthquakes. Comparisons between records obtained from instruments on alluvial soil and on rock not too far away are likely to be of value but there is no certainty that the same relations will hold for a larger area. Assuming that a certain area and a given material vibrates as a whole in its own period, it seems likely that the area and, therefore, the period and amplitude would vary with the intensity of the earthquake. This is a problem very similar to that of the use of models in shaking-platform tests which has been discussed.

I see the earthquake problem as one great problem the parts of which are all interrelated. Some portions of the problem seem far removed to others, but I think that the greatest progress will be made if we fix certain broad aims. As I see them these include: (1) Knowledge of earthquakes, place of occurrence, frequency and intensity--accurate information of this sort is fundamental; (2) cause of earthquakes--this includes combination of observational data and theory, the observational data to include both observations of near and distant earthquakes; (3) nature of activity in central region--

includes both material needed by engineer and questions of propagation, the solution of which is urgently needed by the engineer; (4) knowledge about the interior of the Earth--of interest to the astronomer, seismologist, and geologist and derived from both near and distant earthquake-records; (5) simulation of earthquakes to study effects on structures--accuracy of simulation and the question of relation of models to full-size structures and interpretation is field of engineer.

U. S. Coast and Geodetic Survey,
Washington, D. C.

A SERVICEABLE SCALE FOR EARTHQUAKE-INTENSITY

Alexander McAdie

In the leading European scientific weekly (Nature, Feb. 22, 1930, p. 292) we read that "the Riviera Earthquake (Feb. 23, 1887) was not one of great strength for only 640 persons were killed, while the area disturbed contained 219,000 square miles."

This certainly is a new way to estimate earthquake-intensity, namely, by the number of deaths per unit area and at first reading we are disposed to smile at the crudity of the classification; for do we not all know that the number of fatalities may be greatly influenced by factors other than earth-movements. The loss of life in Tokio and Yokohama must be attributed to fire, and congestion in fire-swept zones; and while the earth-movements were of almost maximum intensity the loss of life might have been comparatively light. It is safe to say that hereafter in Japan and elsewhere, the authorities will take rigorous measures to prevent overcrowding and exposure to danger.

On the other hand, the San Francisco earthquake would have had a tenfold, perhaps fiftyfold, death list had the earthquake occurred four hours later, when the schools were in session--for nearly every school-building was demolished; or eight hours earlier when the theaters and music-halls were crowded.

But as a matter of fact, although entirely unscientific, the popular estimate of an earthquake does depend upon loss of life rather than damage to property, or acceleration or amplitude of displacement of the earth-particle or period or resonance, nor will a strictly scientific classification based upon this last-named amplitude of the curve representing movement of earth-particle be satisfactory; for as was pointed out in the discussion of a proposed scale based on acceleration-values, there is no sure way of telling how these values vary with distance from origin, character of surface-wave, the orientation of the seismographs, or the periodicities. In connection with the installation of seismographs, I believe it is now recognized that it makes quite a difference in the intensity of the record if the wave comes head-on, so to speak, or reaches the instruments at an angle--broadside, we may say.

But if it is thus difficult to obtain true records--by which we mean records without the embroideries due to various causes--how much more difficult must it be to interpret rightly observations made at sea; and we are now at a point where some attention should be paid to ocean-reports. In connection with weather-reporting, an additional word in messages by radio tells when sea-shocks are felt. This is a decided step forward, and it now remains for a society like the Seismological Society of America to further the work, either by urging the installation of seismographs aboard ships, or at least by getting out informa-

tion sheets whereby navigating officers may understand more clearly what sea-shocks are, where they are likely to occur, and the importance of details.

And before we leave this phase of the general problem, would it be worth while to try to persuade the Hydrographic Office to enter on some map the contours of the ocean-bed or at least some of the more prominent features, looking forward to the time when main faults may be outlined more or less definitely. This also affords me the chance to call the attention of both the Coast and Geodetic Survey and the Geological Survey to the interest and value which would attach to maps showing faults along our eastern seaboard. The Seismological Society of America has attempted this (I think great credit is due to the Society and the cooperating bodies) for California. Can we not do as much for this side of the continent? I recognize the difference in the ages of the two sides of the continent, but, even so, there may be points of great interest in an eastern fault-map.

While it is fully recognized that objection can be made to the employment of acceleration at any given point as a basis for estimating earthquake-intensity, in view of the fact that no other quasi-scientific scale has been suggested, we offer the following.

The advantages offset, we think, the objections. First, it is a decimal scale, the minimum value 1 representing an acceleration of one millimeter per second per second, and the maximum 10 an acceleration of 10,000 millimeters per second per second, which with a little squeezing is practically the acceleration of gravity at sea-level in latitude 45°. If we use the new dynamic meter, the scale fits nicely. The scale is in accord with C.G.S. units, a great gain, and of course international in character.

If we manufacture the word kilogal to represent the extreme acceleration in a catastrophic earthquake, the prefix kilo meaning one thousand, and the second syllable being the first letters of Galileo, we can likewise use milligal for the feeblest acceleration, the range being one million units of acceleration.

Proposed scale for earthquake-intensity
(Compare with Mercalli-Cancani intensity-scale)

Intensity	Description	Acceleration	
		milligals	mm/sec²
1	Earthquake detectible only by instruments	1 to 5	0.01 to 0.05
2	Very feeble	5 to 10	0.05 to 0.10
3	Feeble	10 to 50	0.1 to 0.5
4	Noticeable but no damage	50 to 100	0.5 to 1.0
5	Felt generally	100 to 500	1 to 5
6	Slight damage	500 to 1,000	5 to 10
7	Walls cracked	1,000 to 10,000	10 to 100
8	Badly built houses destroyed	10,000 to 50,000	100 to 500
9	Violent; much destruction and loss of life	50,000 to 100,000	500 to 1,000
10	Catastrophic	100,000 to 1,000,000	1000 to 10,000

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A TILT-COMPENSATION SEISMOMETER

H. E. McComb

It is a well-known fact that horizontal-component seismometers are affected by changes in the inclination of the axis of rotation of the boom whether this change in inclination is caused by temperature-changes of the instrument itself or by changes in level of the pier due to tilting of the ground, the building in which the instrument is located, or to other causes. In the case of instruments using galvanometric registration such as the Wenner or the Galitzin, this is not a serious problem as the galvanometer is practically unaffected by tilt. However, for those instruments in which direct registration is used, whether mechanical or photographic, the problem of slow tilt may become so serious as to result in partial or total loss of record from overlapping of the lines on the seismogram or from excessive spreading. This is a real problem at the Coast and Geodetic Survey station at Chicago where it is necessary to make two adjustments of the light-spots daily in order to keep the record within the limits of the drum. At Honolulu the trouble was partly eliminated by alteration of the direction of translation of the recording drum.

After giving this subject careful consideration it was decided that there was need of a seismometer of moderate magnification (100 or less), which would record practically all of the major quakes, would not be put out of commission by the lighter local disturbances, and would be practically free from the effects due to slow tilt. It seemed that the simplest method for making a preliminary test of such a seismometer was to apply to existing equipment a modification of a compensation-device used by Dr. Romberg at the University of Hawaii several years ago. Accordingly, a 10-kilogram Bosch-Omori horizontal pendulum was modified for photographic registration and tilt-compensation. No changes were made in the column, upper or lower supports, pivots, or boom or steady mass. The balance of the instrument was entirely rebuilt.

The desired magnification is obtained by means of a combination of optical lever and short multiplying-lever coupled to the steady mass through oil (see Fig. 1). Light from a galvanometer-lamp is reflected downward vertically by a prism through a plano-convex lens of one-meter focal length and after collimation falls upon a plano-parallel mirror facing upward. After reflection back through the lens and prism it is brought to focus on a recorder. The mirror is attached to a very light multiplying-lever pivoted at two points very close to the plane of the mirror. The other end of the lever-arm is attached to a light, brass vane about one cm square which normally lies in a vertical plane through the pivots. The vane is braced for rigidity in a direction at right-angles to the plane through the vane. The multiplying-lever when oscillating freely upon its pivots has a natural period of about 0.2 second. It is coupled to the steady mass through heavy oil contained in a trough the cross-section of which is slightly larger than the area of the vane. For periods varying in length from a very small fraction of a second up to 30 or 40 seconds the vane follows the steady mass without appreciable loss of amplitude. But for very slow tilt such as may be caused by diurnal variation in temperature or to other causes, the multiplying-lever compensates continuously and the mirror remains practically in the same horizontal plane.

Some undamped decay-curves have been recorded by this instrument and it is found that the static friction is very small. Starting from an initial trace amplitude of about 20 cm the system will oscillate for almost an hour before the trace-amplitude dies down to two-millimeters, the system operating at a natural period of twelve seconds and nominal static magnification of about 100

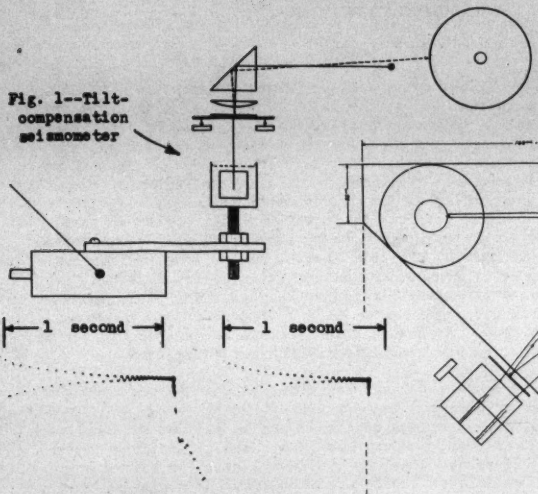

Fig. 1--Tilt-compensation seismometer

Fig. 5--Plan for mounting two horizontal-component seismometers

Fig. 3--Record of rapid oscillations made by tilt-compensation seismometer

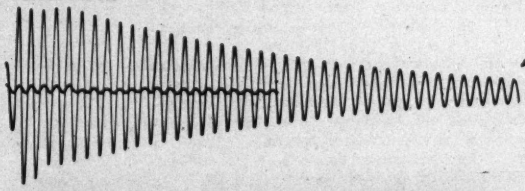

Fig. 2--Decay-curve made by tilt-compensation seismometer

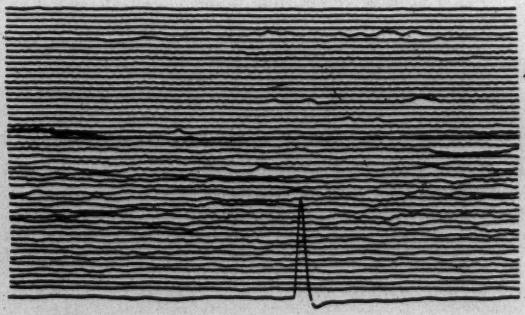

Fig. 4--Record by tilt-compensation seismometer October 1, 1929, showing damping-test and microseisms

(see Fig. 2). The system will also measure earth-displacements or oscillatory movements of fairly high frequency (see Fig. 3). In the illustration the interval between vertical lines is about one second and the recorded curve represents the natural period of the support carrying the multiplying lever. This has a natural period of about one-fiftieth second. This record seems to leave little doubt as to the ability of the system to record fairly rapid ground-motion.

In the original model it was necessary to provide oil-damping as the steady mass is ten kilograms. This method is quite effective as shown by the damping tests (see Fig. 4). The instrument has been operated at a damping ratio of about 20 to 1, although it is easy to provide critical damping if desirable. The record also shows the microseismic motion and the effectiveness of tilt-compensation. The actual distance between successive lines on the original record is only two mm. The other disturbances on the record are entirely local.

Two horizontal components may be recorded conveniently on the same drum by providing an extra reflecting prism on each support and placing the recorder between the columns with its axis at 45° to the booms (see Fig. 5). It is of course understood that this is a very desirable feature especially when it becomes necessary to interpret the gram.

During the past six months this instrument has been in operation for short periods and several teleseismic records obtained. Unfortunately it was not in operation during the Grand Banks disturbance last November.

In some of these tests a clock-driven recorder recently developed in Washington was used. This recorder has a fairly open time-scale and is very suitable for teleseismic registration.

It is believed that this type of tilt-compensation may be applied to a low magnification, vertical-component instrument. Tests are being conducted with this in view. Also it is believed that a horizontal-component instrument of much smaller dimensions and equipped with tilt-compensation of this kind would function properly. In this case magnetic damping could be used instead of oil-damping as at present.

U. S. Coast and Geodetic Survey,
Washington, D. C.

PROGRESS IN AMERICAN SEISMOLOGY

A. L. Day

Ten years ago, it is easily within the memory of all of us, there was very little activity in seismology in this country. I think it fair to say that of coordinated interest or of systematic research there was substantially none within the United States. Some very well-equipped individual stations were maintained in different parts of the country by the Jesuit group for purposes of record and those were, I think, the only competent ones in regular operation in this country at that time. Each one of these stations was separately maintained and quite independent of the others both administratively and scientifically and even the reports of that time did not employ a common terminology. The Jesuit Seismological Association, which provided an effective organization of these stations into a well-coordinated group with a strong common purpose and a research-program, is of later date.

The Weather Bureau had a number of instruments in various places operated by volunteer observers not particularly trained or interested in the subject. The seismologic service in the Weather Bureau was somewhat local and adventitious in character and probably owed its existence to the personal interest of the Chief, Dr. Marvin, rather than to an authentic public demand. There was no research-program. In Canada a much more consistent effort had been made to develop the research side of the problem, though with limited means and a somewhat restricted program when compared with England or the Continent. The plans subsequently matured by the Carnegie Institution of Washington were not even under consideration at that time, ten years ago.

It was therefore quite possible for H. O. Wood to say in a report to the National Research Council about that time that seismological research in the United States was negligible. He called attention particularly to the efforts that had been made during the previous ten years in Japan, England, and Germany and to the stations maintained in other parts of the world, including Spain and the Philippines, all of outstanding quality when compared with any in this country. He therefore urgently pressed the need for more attention to American seismology. The United States had had its share of the calamities but was making no efforts in its own behalf either to discover their causes or to provide protection for its inhabitants in exposed places.

Wood found a rather barren field--barren in experience and barren in interest. I am not fully informed of the details of his effort though he personally appeared before several bodies and his written recommendations were offered to the National Research Council and to some of the other research organizations. So far as I recall no one was found whose interest approached in magnitude the two million dollars proposed for the inauguration of his project. Nevertheless, the presentation which he made took root in a small way in several directions. I was personally delegated by President Merriam of the Carnegie Institution of Washington to make a canvass, of which this may serve as a brief review, to discover exactly what efforts were being put forth in seismology in this country, whether they were properly coordinated and supported, whether anything could be done to bring the interested institutions together in a common effort, and finally whether anything should be done to supplement that effort. At the close of this canvass recommendation was made that the Institution itself take the initiative by undertaking a particular problem in this field of activity following substantially Wood's proposed program of California fault-zone studies and endeavor to bring about a more effective coordination of effort and interest in the research side of the subject. The "Advisory Committee in Seismology" was organized and has since continued to serve under the chairmanship of the present speaker, as a kind of clearing-house for the consideration of American seismological questions in so far as the Carnegie Institution of Washington might become a participant in the general development-program.

One conspicuous feature of this report brought out the fact that no instrument was then available through which to determine local epicenters in California, the region in the United States to which attention was most obviously directed. The effort was accordingly made by the Advisory Committee to call together a group of specialists to consider this need and to recommend a sound design for such an instrument. This effort eventually led to the present Wood-Anderson torsion-seismograph, though by ways which were long and sometimes devious. But fortunately the progress of an undertaking is measured by the results and not by the number of steps taken to attain them. The assurance that success had been finally attained was perhaps most securely indicated by an incident which gave great pleasure in Pasadena last October. When Father Macelwane received word that a sum of $4,000 had been awarded to him by the National

Research Council for equipment for his studies of earth-movements in the Mississippi basin, he elected to use and at once placed orders with the makers for the Wood-Anderson seismograph. Intimately associated with this instrument throughout its development was Dr. Frank Wenner of the Bureau of Standards. No sooner had the short-period Wood-Anderson instrument come into successful use than he set about independently to devise an equally simple long-period instrument through which to supplement it for the record and study of long-period earth-waves. This instrument has proved equally successful. Indeed if the effort so far made had produced no more than these two instruments the contribution would have been a notable one.

Other steps in the advancement of the project were directed rather to arouse a more active interest in the subject among existing agencies and to secure better coordination of effort. The Weather Bureau, in particular, as the only legally empowered government agency was appealed to for further efforts but it was presently shown to be unwise, possibly out of the question, to secure from the Weather Bureau any more active interest in seismology than had already been shown. The farmers' interest (the Weather Bureau is in the Department of Agriculture) is in the weather, not in earthquakes, so that the Weather Bureau could not be asked to set aside any considerable sums from its limited resources towards the advancement of an unsympathetic project. With the full consent of the interested government officials and appropriate action by Congress, seismology was accordingly arranged to be transferred to a bureau in which the interest was greater and the standards of precision of the highest. So far as the Government is concerned, seismology is now most appropriately housed in the Coast and Geodetic Survey, and the activity of this group is measurably indicated by the reports from Commander Heck's division supplemented by his personal activities as the most efficient Secretary of the Eastern Section of the Seismological Society of America and the success which it has attained under his leadership. It is further indicated by the deep interest in these problems which the Director of the Coast and Geodetic Survey yesterday expressed in the hearing of all of us, an interest not alone in the survey and appraisal of earthquake-dangers but equally in the research-phases of the problem.

About eight years ago Professor Lawson of Berkeley, California, published a collection of astronomical observations which seemed to indicate that the California coast was moving northward about a foot each year. Closer inquiry into that situation brought out substantially these facts. There were records from the latitude station at Ukiah in California, an international latitude-station by the way, indicating a continuous movement northward but pointing out quite plainly that astronomical observations for fixing the position of points on the Earth do not permit of closer determination than about one meter. The smallest displacement observable by astronomical means therefore represented three years of movement. However, some twenty years of continuous records were available at Ukiah and Lick observatories and Professor Lawson was led to the conclusion that a case had been established in favor of a northward movement of this order of magnitude. Our Committee took a vigorous interest in this conclusion as one of its first activities, Professor Lawson being himself a member of the Committee. Continuation of the Ukiah observations, which were threatened by the consequences of the war, was arranged for--also through the effective cooperation of the Coast and Geodetic Survey. The Lick Observatory observers also cooperated but were never so confident that Lawson's interpretation was the correct one. The error of observation appeared to them too large. It remained for Dr. Lambert to point out that if all the international latitude-observations (Italy, Japan) made along the same parallel of latitude were brought into the picture polar movement was plainly indicated and not a local

northward movement in California.

Another activity of the Advisory Committee in Seismology which has been reported to you on several occasions before, concerned itself with establishing a network of primary triangulation-stations over the State of California. Whether or not the astronomers should succeed in proving a northward movement for the portion of California lying west of the Sierra, the Advisory Committee concluded straightway, with the concurrence of several conspicuous men in political life in California, that it was wise to begin an accurate survey of the earthquake-zone in that state. A crustal movement of a foot a year has great economic consequences in such areas as these and no effort is too great to establish the truth or falsity of such hypotheses. Again the Coast and Geodetic Survey gave its full support to this plan and Congress provided annual appropriations--at first of $15,000, but since reduced gradually as the work progressed--for the prosecution of a survey of such precision that earth-movements amounting to no more than one part in 200,000, or one foot in a triangle whose sides are 40 miles long, could be detected and measured. This survey has sufficed to tie down the intersections of the California triangulation-net with that order of accuracy. The network begins somewhat east of Reno, in a region believed by the geologists to be stable, and extends westward to San Francisco. From there it continues both north and south to cover the entire range of known movement along the San Andreas fault from the Pacific Ocean at Point Arena to the Mexican boundary. At the southern end it is likewise anchored by a loop eastward into the Colorado River basin. Later there was a still further tie-in of this network with the general triangulation-system of the Coast and Geodetic Survey extending westward from Salt Lake City and reaching the coast about half way between San Francisco and Los Angeles (at San Luis Obispo).

In the consideration of this program it was at first suggested that vertical movements were not to be expected in these regions and that precise vertical leveling was therefore not needed in California. Over against this the younger members of the Advisory Committee, notably Drs. Noble and Buwalda, pointed out that vertical movement was of the very essence of the relation between the Sierra Nevada Mountains and their surroundings. Be that as it may, a number of lines of precise levels have already been run in the fault-zone region of Southern California. Perhaps it is significant in this connection and perhaps not, but it may be of interest to you in bringing this matter up to the moment of speaking, that there has been this year some evidence of movement in and about the Salton Sea following the strong shocks of January and February in that region, and it appears to be vertical movement and not horizontal. Existing levels in that region will be checked during the coming summer (1930).

The Hydrographic Office of the Navy was invited to collaborate in an effort to ascertain whether the known faults of California extend beyond the coast-line and whether any of the earthquakes which are recorded in California have their source somewhat off the coast. The new sonic sounding-device was available. The Navy was willing to direct its practice cruises into those waters and the effort was made to run depth-determinations outward from the coast in sufficient numbers to permit drawing a contour-map of the ocean-floor from the coast-line to a depth of 12,000 feet.

The Committee, in its effort to increase interest in all questions directly and indirectly pertaining to the advancement of seismology, used its good offices freely in support of activities of other societies interested to promote the advancement of this subject of research. The Jesuit Seismological Association, founded in 1925, perhaps reflected something of this general wish for closer coordination of scattered efforts by providing for an annual conference

and collaboration both within and without the Jesuit group. No general coordination of the Jesuit stations had been attempted up to the time of the first general meeting in Chicago in August 1925. Today it is a most vigorous and forward-looking body.

On the other hand, the Seismological Society of America, legally a California organization and the parent body of the Eastern Section, has developed much more slowly as perhaps should have been expected. Speaking broadly of the membership this organization is not made up of seismologists at all but is a group of broad-minded citizens, willing to admit the earthquake-menace and interested to contribute a small sum each year to provide an organization in the common interest and a modest (quarterly) journal to carry such news as there is of earthquakes and earthquake-studies. Except for a very small minority composing the directorate the members of the Society individually hardly have more interest in it than this. Meetings for the discussion of seismologic events and progress in recent years have been rarely held and scantily attended. Notwithstanding the "Bulletin" maintained by the Seismological Society of America, the publication of research work, now that we have it in active progress at several centers, is somewhat of a problem. Papers from this body (Eastern Section) presented at our session a year ago still remain unpublished in the hands of the Editor of the Bulletin.

More or less under the inspiration of the Carnegie Institution of Washington, there was brought together in Pasadena last year a small group of men actively interested in crustal movement to discuss in round-table fashion the present seismological problem. Professor Jeffreys of Cambridge, Professor Gutenberg of Frankfurt, and a number of others well known to you were gathered there. Every phase of present-day research activity in that field was canvassed by that body. It was a fine showing of the progress of development in modern seismology not only in this country but in Europe. We have reached the firing line in seismology. It remains to observe how we shall comport ourselves there. The success of our effort depends in large measure upon cooperation. The problems of seismology can not be resolved in one spot by one man, but working together in appropriate localities in which earth-movement is known to occur, it will be possible to find out a great deal about them with the apparatus now available. The chief need today is for coordination of effort by and between the Government, the educational and research institutions equipped for such studies, citizens generally who are interested in earthquakes, and finally those of our people (for example, in California) who profess not to be interested but who live beneath a continuing threat of impending danger. Now that interest has been aroused and the path of constructive progress pointed out, all these have a definite share of responsibility.

An interesting story might be told of the sensitiveness and the prompt defense-reaction of the people of Southern California against the encroachment of earth-movements into their domain. When seismologic stations began to be set up in the vicinity and to record officially and permanently such movements they did exactly what you and I would do to protect ourselves against a trespass. They employed an advocate to present a brief for their viewpoint that earthquakes do not visit Los Angeles, and inasmuch as the threat had come through instruments in the hands of students in geology, a well-known geologist was employed to prepare the brief which was finished something over a year ago and has since been widely circulated in that region.

Thus was confidence restored. A seismologic invasion had seemed to threaten, the ablest advocate available had been employed to make a vigorous defense. The defense was now made, was extensively circulated, and the enemy

was now completely vanquished. It mattered little thereafter that our instruments continued to record minor movements at the rate of about 200 per year or that an occasional somewhat stronger shock, like that which centered near the Whittier fault last year, brought down the dishes, and even the chimneys, in the southern portion of the city.

I have not personally been so much concerned to make immediate application of seismologic conclusions to the problems of everyday life as to increase the body of fundamental information about earth-movements which should be available for such purposes. I think it was Sir Oliver Lodge, speaking before the British Association in the early nineties, who said in substance, "There is no lack of able men to turn to practical usefulness scientific principles once established." It is as true today. When the Santa Barbara earthquake brought into confusion the whole system of earthquake-insurance in the United States it was not long before an experienced and sensible man, our speaker of yesterday, Mr. John R. Freeman, found a practical way to distribute the load in such a manner that it would not become burdensome. Yesterday we heard the same gentleman propose a plan of study through which buildings may be appropriately strengthened to withstand earth-movements in those regions where they are to be expected. You recall, however, that at the foundation of his system was an assumption, namely, that the force against which protection is sought is of the order of one-tenth of gravity. It is the seismologist's task to learn more, much more, about the character, the magnitude, and the distribution of earth-movements in different regions of the crust. Can we not substitute measured behavior in place of assumptions?

Geophysical Laboratory,
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Washington, D. C.

THE ATTICA, NEW YORK, EARTHQUAKE

Arthur Keith

THE GRAND BANKS EARTHQUAKE

Arthur Keith

These two communications by Dr. Keith gave preliminary accounts of the two earthquakes; the final papers will be published in the American Journal of Science.

SECTION OF METEOROLOGY

(Washington, D. C., May 1, 1930)

RELATION BETWEEN THE SECTION OF METEOROLOGY OF THE INTERNATIONAL GEODETIC AND GEOPHYSICAL UNION AND THE INTERNATIONAL METEOROLOGICAL COMMITTEE

C. F. Marvin

This statement represents my personal views on this question, without any attempt to coordinate them with others or to ascertain the consensus of opinion concerning which differences of view are sure to prevail.

To understand the question it is necessary to understand the nature of the two organizations. It is assumed quite unnecessary to discuss the nature or organization of the International Geodetic and Geophysical Union, with its sections, especially that of Meteorology. On the other hand, the International Meteorological Organization is not so well known, except among meteorologists, even though it has been in more or less complete existence for more than 50 years. Moreover, its activities have been marked by an unusual degree of harmony and cooperation in the field of meteorology. Therefore, it commands our reverence and respect.

The World War interrupted the activities of this organization, and the proposal to reorganize the International Geodetic and Geophysical Union shortly after the war also seemed in part to occupy the field of the older organization. Nevertheless, the meteorological organization, which up to that time was more or less familiarly known throughout the world, not under the name of the International Meteorological Organization, but rather under the name of the International Meteorological Committee, which was the working agency of the so-called International Conference of Directors, preserved its integrity, and through the medium of successive meetings since the War the organization of the international meteorological activities has been more definitely formulated and strengthened in several ways than ever before. At the last meeting, held at Copenhagen in 1929, this important international body definitely adopted and put into force the name International Meteorological Organization. This organization is rather complex in its composition. It comprises: (1) The Conference of Directors; (2) the International Meteorological Committee; (3) the Secretariat of the Organization; (4) Commissions of the Organization.

Throughout its history the Conference of Directors has aimed to meet at intervals of about six years, and is called together by the International Meteorological Committee. Its membership consists of the Directors of the National Meteorological Services of all the nations, also the Directors of recognized independent meteorological observatories or organizations.

The Conference of Directors has for its principal function discussing administrative questions, means of carrying them out, to keep in touch with methods of observation and of calculation, to decide the ordinary tasks which should be undertaken, and, finally, to create the necessary commissions. Purely theoretical questions are not within the scope of the conferences. The Conference of Directors names the members of the International Meteorological Committee, who are in fact the directors of meteorological services, and the so-called International Meteorological Committee carries on the activities of the organization in the interval between conferences of directors.

In the reorganization since the War, provisions have finally been made also for the Secretariat of the organization, which is really the working office

of the President of the International Meteorological Committee, and which has become necessary in recent years owing to the great expansion of the meteorological work among all nations and the accumulation of correspondence and clerical work incident to the affairs of the Committee. At the Conference of Directors at Copenhagen, decision was reached to have the office of the Secretariat permanently located in Switzerland. The expenses of this office will be covered by voluntary contributions by the different national services in the proportion of 4,000 gold francs for large nations, 2,000 gold francs for those of medium size, and a lesser sum of 1,000 gold francs for other states.

Finally, the commissions of the organization constitute, from my point of view, the only feature of the activities of the International Meteorological Committee which can be considered as involving some possible complications, or duplications, between the Committee and the International Geodetic and Geophysical Union. There are at present twelve so-called commissions, as follows: Commission for Terrestrial Magnetism and Atmospheric Electricity; Commission for Solar Radiation; Commission for Exploration of the Upper Atmosphere; Commission for Synoptic Weather-Reports; Commission for Marine Meteorology; Commission for Reseau Mondial and Polar Meteorology; Commission for Agricultural Meteorology; Commission for the Application of Meteorology to Air-Navigation; Commission for Propagation of Sound; Commission for Study of the Clouds; and Commission for the Study of the Creation of a Bureau of International Meteorology.

The functions of these commissions are to foster, promote, and investigate the subjects, particularly meteorology, designated and comprehended by the names of the organizations. These are really technical working subcommittees, and are occupied to a very great extent in working out the details of methods and facilities for securing meteorological observations, statistics, etc., on a homogeneous, world-wide scale, including, of course, the technical investigation and studies incident thereto.

In contrast to the work of Section C of the International Geodetic and Geophysical Union, these commissions, consisting of eminent scientists in their respective fields, deal largely with the practical problems rather than the abstract and theoretical problems of a more or less purely scientific character, including, of course, the correlation and coordination of these meteorological questions with related questions in the general field of geophysics. These latter are functions of the International Geodetic and Geophysical Union. From my understanding of the activities of the two organizations, I believe that in the last analysis it will be found very little duplication will arise unless the Geodetic and Geophysical Union should independently organize work corresponding to one or more of the commissions of the International Meteorological Organization.

There are so many strong reasons why the commissions of the International Meteorological Committee, composed as they are of officers and important officials of the existing meteorological services, are in the best position to carry out the work in their respective fields that it is believed they should be allowed to exercise their functions with a relatively free hand, and that activities in these fields to be undertaken by agencies of the Geodetic and Geophysical Union should carefully avoid material duplication of those of the meteorological committees, or that the two agencies should carefully coordinate and affiliate in work that may be more or less in common.

As stated at the beginning, these are the personal views of the writer concerning the relationship involved, and I feel that the International Meteorologi-

cal Organization, by reason of its intimate identity with the national services of nearly all nations, as well as by its age, is entitled to recognition on the part of the Section of Meteorology in the Geophysical Union as the active, existing organization in the field.

U. S. Weather Bureau,
Washington, D. C.

THE WEEK AS A UNIT OF TIME

C. F. Marvin

Progress in many branches of geophysical research involves the critical statistical analysis of great masses of homogeneous data like those of meteorology, solar physics, vital, health, and other statistics which for the purpose must be assembled in appropriate time units of days, weeks, months, seasons, years, etc. It is surprising that many investigators who are meticulous about many details will nevertheless complacently tolerate the inherent inaccuracies caused by the use of the present calendar with its unequal months and the fractional relationships of such subunits as weeks and months, or decades with months or years, etc.

Whatever explanation, religious or otherwise, may be advanced for the existence of the seven-day week, it is the only subunit of the common calendar next greater than the day which is as nearly as nature permits an exact submultiple of the year. Moreover, its multiples by two and four are also exact aliquot parts of the year as nearly as nature permits, which is distinctly less true for any other customary calendar-unit.

Although the pentad is, truly enough, an exact aliquot part of the year, nevertheless it has serious disadvantages. First, except in Soviet Russia, it is not in civil use, and in general it is too short, but worst of all none of its multiples is an aliquot part of a year. Known and advocated for many years as a small unit for summarizing meteorological data, it has never won favor. On the other hand, the seven-day week, with its multiples the fortnight, the four-week month, the thirteen-week quarter, the 26-week half-year, and the thirteen 28-day months, continue with single days as nearly an ideal system of calendar-units in equal and aliquot relations as nature permits under the circumstances.

Repeating an obvious analogy, our calendar is a "time-stick" for measuring the passage of days, weeks, months, and years. How long would science tolerate yard- and meter-sticks having important subdivisions unequal and in fractional relations to each other? May we not hope the needs of industry, business, and science will prevail over ultra-conservatism and superstition and soon secure the introduction of a rational calendar constructed on scientific lines?

Summaries of meteorological statistics in seven-day weeks, the fortnight, and in four-week months are greatly needed but can not now be secured from the ordinary monthly summaries, and the writer urges consistent international effort to inaugurate simplification of the calendar and some plan for the orderly preparation and publication of statistical data to make such summaries available. For this purpose the seven-day week must be the basis of tabulations.

Two plans for effecting quarterly division of the year demand consideration.

Schedule of weekly statistical tabulations for climatic year of temperate zone

Season	Date, first day of week Gregorian calendar	Remarks	Season	Date, first day of week Gregorian calendar	Remarks
Winter	Dec. 3		Summer	June 4	
	10			11	
	17	Winter solstice in this week		18	Summer solstice this week
	24	Week may contain 8 days		25	
		Old year ends, new year begins			
	Jan. 1			July 2	
	8			9	
	15	Mid-week of winter 4 weeks		16	Mid-week of summer 4 weeks
	22	after solstice		23	after solstice
	29			30	
	Feb. 5			Aug. 6	
	12			13	
	19			20	
	26	This week may contain 8 days in leap-years		27	
Spring	Mar. 5		Autumn	Sep. 3	
	12			10	
	19	Vernal equinox in this week		17	Autumnal equinox in this week
	26			24	
	Apr. 2			Oct. 1	
	9			8	
	16	Mid-week of spring 4 weeks		15	Mid-week of autumn 4 weeks
	23	after equinox		22	after equinox
	30			29	
	May 7			Nov. 5	
	14			12	
	21			19	
	28			26	

The climatic seasons differ from zone to zone on the globe, and none of them fit either the calendar or the astronomical phenomena of the planetary year. More or less arbitrary compromises are therefore necessary.

If we think in world-wide units, the equinoxes and solstices form rational quarter-points for centering meteorological data. If we recognize the claims and interests of the vast population of the temperate zones, we notice the climatic seasons lag for the most part just about four 7-day weeks behind the astronomical seasons.

No argument is required to establish the necessity that in making the week a time-unit the first week of the calendar-year must necessarily always begin with January 1, otherwise some weeks will be split with part in one calendar-year and part in the one before or after. Some further convention must be made to dispose of leap-day and the 366th day over 52 weeks in common years. The simplest plan of disposing of this inherent necessity is to have once a year and twice in leap-years weeks with eight instead of seven days when circumstances require, but the exact method of adjusting statistics to this requirement may best be left to the merits of individual cases.

The foregoing furnishes a mere outline of the advantages and methods of utilising the week as a statistical unit for tabulating data of observations such as arise in meteorology. Supplied with data of this character the investigator can easily form combinations, like fortnights, months, etc., to suit the particular requirements of his investigation.

U. S. Weather Bureau,
Washington, D. C.

GEOPOTENTIAL AND GEODYNAMIC METERS

W. J. Humphreys

The work done in lifting a mass m, h units of length vertically, is given by the equation

$$w = m\,g\,h$$

provided g, the local gravity-acceleration, is constant over the distance in question. If g varies with height, and we know it does, then we must resort to integration and write

$$w = m \int_0^h g\,dh$$

and express g as a function of h. When m is the unit mass and the initial or zero-height is sea-level the value thus obtained for w is the gravity-potential, or "geopotential," at the height h at the given locality.

Obviously, if g were the same at sea-level everywhere, and everywhere varied the same way and to the same extent with equal increase of height, then any equipotential surface would be at the same height above sea-level at all places, and any two such surfaces would everywhere be equally distant from each other. But g has no such constancy, either in value or variation, hence equipotential surfaces are not geometrically parallel to each other. Evidently we can express heights in terms of linear units, just as we measure horizontal distances, and thus fix a point in the air by its linear height above sea-level at a particular

latitude and longitude, or we can fix it equally definitely by latitude, longitude, and geopotential. If differences in geopotential are used instead of differences in height it is necessary, of course, to express their values in terms of some potential unit. One of the most extensively used of these units is 10^5 ergs, or the work necessary to lift one gram a little more than one meter. This is called the "geodynamic meter," though it is not a meter in any sense of the term, but just an arbitrarily chosen "unit" of energy.

For certain studies of the atmosphere and its movements equipotential surfaces and geodynamic heights are essential. But surfaces of equipotential temperature, or isentropic surfaces, also are useful; and so too are surfaces of equal density; and likewise equal this, that, or the other, according to the particular problem in hand. It therefore would be ideally convenient for users of free-air data if the "heights" were expressed in all these several ways. However, so great an elaboration is impractical, therefore heights nearly always are given in terms either of linear units or of potential units--meters or geodynamic meters, for instance. Commonly, too, the coefficient that transforms the numerical value of a particular height in terms of either unit into that in terms of the other is known to an approximation close enough for practically all needs. Clearly, then, it is largely a matter of choice as to which of these two units we shall use in recording the data of free-air observations. Presumably, however, most people would prefer the meter, or some other linear unit, since this conveys to one a perfectly definite and familiar idea, while height in terms of geodynamic meters is apt to leave one more or less uncertain as to exactly what is meant, if not even bewildered. And there are other advantages. For example, the height of a cloud-base in terms of geodynamic meters could give the aviator, for instance, some idea of the amount of fuel needed to climb to that ceiling, but it is the height in linear meters, not geodynamic, that tells him what will be the possible reach of his horizon, and whether or not a neighboring mountain can be safely cleared, matters of much greater importance to him than the theoretical one of the fuel-consumption necessary to reach a given level--a mere question at most of varying the throttle, so far as he is concerned.

Each of these height-units, the linear and the geodynamic, is essential in its own field, but except where the need of the latter is clearly indicated the former would seem likely to be the more widely understood and generally useful.

U. S. Weather Bureau,
Washington, D. C.

THE EFFECTS OF SEVERAL DIFFERENT DEPTHS OF FUNNELS ON THE COLLECTION OF RAINFALL

Benjamin C. Kadel

In accord with a request by Dr. Kimball dated December 4, 1929, for suggestions as to what the United States delegation to the Stockholm meeting should contribute to the discussion of Resolution No. 5, Messrs. Hayes, Kadel, and Fergusson made the following statement:

> "In specifications for a standardized rain-gage particular attention should be given to the depth of upper flange of funnel, and the shape and slope of funnel, to prevent outsplashing; and to the minimum diameter of funnel; also that there be provided a narrow-mouthed con-

tainer for collected water, to minimize losses by evaporation."

The occasion seems appropriate to present some observed facts that bear upon this recommendation.

The standard 8-inch rain-gage of the United States Weather Bureau is equipped with a collecting funnel having a vertical wall two and one-quarter inches deep to the sloping part, then a slope angle of forty-one and one-half degrees below the horizontal to the outlet.

From time to time honest doubts as to the sufficiency of the depth of this funnel have been communicated to the Instrument Division of the Weather Bureau, and in an effort to obtain some facts several comparisons were carried on.

The first comparison was voluntarily undertaken by Mr. C. A. Hurlbutt, Cooperative Observer, Elk Creek Station (Pine Grove, Colorado), who was provided by the Instrument Division with a second gage exactly like his standard, but with the vertical wall of the funnel six inches deep as compared with two and one-quarter inches for the standard. Mr. Hurlbutt made readings of both gages daily, May to October 1923. The total catch in the standard funnel was 23.59 inches, and in the funnel with six-inch wall, 23.93 inches, an increase of 1.4 per cent. Of the 97 comparisons made, 78 showed exact agreement between the two gages; 7 showed 0.01 inch more caught in the deeper funnel; 7 showed 0.02 inch more; one showed 0.03 inch more; one showed 0.05 inch more; while one instance showed 0.03 inch less.

Differences for each rain can not well be expressed in percentages, and it seems needful for a complete understanding to present, as Table 1, the tabulated measurements as Mr. Hurlbutt reported them. Wind-velocity was not recorded.

Through the cooperation of Dr. Oliver L. Fassig in charge of the San Juan (Porto Rico) station of the Weather Bureau and his assistants a more extended set of comparisons was carried out on the grounds of the San Juan Weather Bureau station. Two standard eight-inch gages, one with vertical wall of funnel two and one-quarter inches deep, the other with wall six inches deep were exposed side by side. Detailed measurements are presented in Table 2. The total catch in the shallower-walled funnel was 47.41 inches and in the larger-walled funnel 47.96 inches or 1.2 per cent more. Of the 145 measurements made, 77 showed exact agreement; 23 showed 0.01 inch more for the deeper funnel; 12 showed 0.02 inch more; 5 showed 0.03 inch more; and one showed 0.06 inch more; 20 showed 0.01 inch less; 2 showed 0.02 inch less; and one showed 0.04 inch less.

A recording anemometer in operation at San Juan makes possible examination of the catch as affected by wind. On nine occasions during which maximum winds 35 to 42 miles an hour were recorded, the catch was 3.55 inches and 3.62 inches, respectively, an increase of 2 per cent for the deep funnel. On 21 occasions with maximum winds 30 to 34 miles an hour, the catch was 5.18 and 5.54, respectively, an increase of less than seven per cent.

Presentation of all these details is hardly warranted, but Table 3 shows that in moderately windy weather the advantage of the deeper funnel is not completely established. Two occasions, February 16 and 17, show an advantage for the deeper funnel, but the total amount of rainfall is not large, nor are these two occurrences considered conclusive evidence.

Table 1--Daily catch of rainfall in 8-inch gages equipped with 2 1/4-inch (A) and 6-inch (B) funnels, respectively, at Elk Creek Station at Pine Grove, Colorado

Day	May		June		July		Aug.		Sep.		Oct.	
	A	B	A	B	A	B	A	B	A	B	A	B
	inch	inch	inch	inch	inch	inch	inch	inch	inch	inch	inch	inch
1					.37	.38	.75	.75	.02	.02	.10	.12
2					.05	.05					.08	.09
3	.03	.03	.20	.20	.02	.02	.01	.01			.23	.25
4			.02	.02	.01	.01	.11	.13			.64	.67
5	.09	.09	.22	.22			.05	.05	.02	.02	.06	.08
6	.03	.03	.07	.07			.25	.25				
7	.02	.02	.05	.05	.37	.37	.81	.85				
8			.55	.55	.53	.55	.36	.36				
9			2.02	2.02			.26	.26				
10			.10	.10	.21	.21	.05	.05				
11	.04	.04					.45	.45	.02	.02		
12	.08	.08	.02	.02	.30	.30	.32	.32			.68	.68
13	.10	.10							.07	.07	.08	.09
14	.29	.31			.36	.36	.15	.15	.10	.10		
15	.23	.23			.72	.72	.55	.55				
16	.01	.01	.20	.20	1.25	1.30	.34	.34	.03	.03		
17	.03	.03			.54	.54	.25	.27	.12	.12		
18							.04	.04	.43	.47		
19					.29	.29	.42	.43			.01	.01
20	.01	.01			.06	.06	.20	.20				
21	.01	.01	.13	.13	.36	.36	.16	.16				
22	.35	.32					.18	.18				
23	.07	.07					.55	.55	.20	.21	.23	.23
24							.05	.05			1.28	1.28
25	.17	.17			.06	.06	.07	.07			.14	.14
26					.53	.54						
27					.95	.96	.01	.01				
28					.08	.08			.21	.21		
29											.05	.05
30									.04	.04		
31							.06	.06				
Tot.	1.56	1.55	3.68	3.68	7.06	7.16	6.45	6.54	1.26	1.31	3.58	3.69
(B-A) inch	-.01		.00		+.10		+.09		+.05		+.11	
(B-A) per cent	-0.6		±0.0		+1.4		+1.4		+4.0		+3.1	

Table 2--Daily catch of rainfall in 8-inch gages equipped with 2 1/4-inch (A) and 6-inch (B) funnels, respectively, at San Juan, Porto Rico, May 6, 1924, to March 23, 1925

Day	May		June		July		Aug.		Sep.		Oct.	
	A	B	A	B	A	B	A	B	A	B	A	B
	inch	inch	inch	inch	inch	inch	inch	inch	inch	inch	inch	inch
1							.04	.04				
2					.21	.21	.24	.24				
3					.04	.04						
4					1.22	1.24						
5			.11	.11	.08	.08					.87	.89
6	.10	.10			1.23	1.23	.02	.02			.20	.20
7			.12	.11	.98	.98						
8			.07	.07								
9			.18	.18								
10	.31	.32	.33	.33								
11	1.72	1.78			.06	.06						
12	.33	.32	1.21	1.21	.01	.01						
13	.06	.07	.08	.08	.18	.18						
14	.03	.04	.06	.06	.09	.09						
15	.11	.12										
16	.07	.07										
17	.53	.53	.06	.06								
18	.08	.08										
19	.15	.15	.01	.01								
20	.15	.15			.13	.13						
21			.69	.67	.28	.28						
22			1.66	1.62	1.18	1.19						
23			.14	.15					1.96	2.00		
24					.07	.07						
25	.04	.04	.36	.34	.07	.07						
26	.03	.03	.15	.15								
27	.06	.06	.04	.04	.03	.03	.42	.44				
28	.12	.12	.02	.02	.24	.26						
29	.21	.21	.07	.06								
30	.10	.10	.09	.08	.21	.21						
31												
Sums	4.20	4.29	5.45	5.35	6.31	6.36	0.72	0.74	1.96	2.00	1.07	1.09
(B-A) inch	+.09		-.10		+.05		+.02		+.04		+.02	
(B-A) per cent	+2.1		-1.8		+0.7		+2.7		+2.0		+2.0	

Table 2--Daily catch of rainfall in 8-inch gages equipped with 2 1/4-inch (A) and 6-inch (B) funnels, respectively, at San Juan, Porto Rico, May 6, 1924, to March 23, 1925--Continued

Day	Nov.		Dec.		Jan.		Feb.		Mar.	
	A	B	A	B	A	B	A	B	A	B
	inch	inch	inch	inch	inch	inch	inch	inch	inch	inch
1	1.94	1.93			.37	.40			.22	.22
2	.15	.15			.12	.13			.02	.03
3			.41	.40	.10	.11	.01	.01		
4			.25	.25	.36	.36				
5	.63	.63	.10	.10	.02	.02				
6	.02	.02	.29	.28	.45	.45	.05	.04		
7					.04	.04			.01	.01
8					.06	.06	.01	.01		
9							.30	.30	.01	.01
10					.36	.37	.94	.98	.06	.06
11	1.21	1.20								
12										
13			.73	.74					.65	.65
14	1.40	1.40					.60	.62	.04	.04
15	.20	.20	.24	.24			.05	.05		
16	.32	.31	.34	.33	.28	.30	.37	.39		
17			.45	.47	.28	.29	.50	.53	.05	.06
18	1.01	1.05	.59	.62			.60	.61	.06	.06
19			.33	.36			.08	.09	.10	.09
20	1.46	1.45	.11	.13	.05	.05	.04	.04	.04	.03
21	2.46	2.50	.29	.31					.08	.08
22	.32	.32	.05	.06	.12	.11	.20	.21	.05	.06
23			.02	.02	.03	.02	.02	.02	.05	.05
24	.31	.31	1.03	1.07			.10	.10	-	-
25			.19	.20					-	-
26			.72	.74	.02	.02			-	-
27			.16	.17	.12	.12			-	-
28			.64	.63					-	-
29			.29	.30	.22	.23			-	-
30			.12	.14	.03	.02			-	-
31			.56	.55	.01	.01			-	-
Sums	11.43	11.47	7.91	8.11	3.04	3.11	3.87	4.00	1.44	1.44
(B-A) inch	+.04		+.20		+.07		+.13		0	
(B-A) per cent	+0.4		+2.5		+2.3		+3.4		±0	

Table 3--Comparison of rainfall-catches at San Juan, Porto Rico, by standard and deeper rain-gages

Date	Wind in 24 hours	Wind maximum an hour	Rainfall		Difference in standard
			2 1/4-inch funnel	8-inch funnel	
	miles	miles	inches	inches	per cent
May 11, 1924	253	15	1.72	1.78	+ 2
June 22, 1924	206	20	1.66	1.62	- 2
January 1, 1925	369	32	0.37	0.40	+ 8
February 16, 1925	620	33	0.37	0.39	+ 5
February 17, 1925	521	31	0.50	0.53	+ 6
Totals.			4.62	4.72	+ 2.2

The San Juan comparisons include also measurements of the catch in two gages 12 inches in diameter, one being the tipping-gage employed by the Weather Bureau for automatic-record purposes and equipped with a funnel with rim three inches deep, sloping down 45 degrees to the outlet; the other differing only in depth of funnel, which was eight inches instead of three inches. The catch in these two gages is shown by Table 4 to be in close agreement throughout the period, some of the monthly totals being the same for both gages, and the totals for the entire period differing by only six-tenths of one per cent. Reference to the individual values, not here presented, show the usual day-to-day differences.

Table 4--Catch of rainfall as given by 12-inch gages equipped with 3-inch (C) and 8-inch (D) rims, respectively, at San Juan, Porto Rico, May 6, 1924, to March 23, 1925

Month	C	D	Difference (D - C)	
1924-25	inches	inches	inch	per cent
May	4.17	4.21	+.04	+ 1
June	5.37	5.35	-.02	0
July	6.33	6.36	+.03	0
August	0.73	0.74	+.01	+ 1
September	2.00	1.98	-.02	- 1
October	1.08	1.08	0	0
November	11.30	11.30	0	0
December	7.95	7.85	-.10	- 1
January	2.97	2.89	-.08	- 3
February	3.86	3.77	-.09	- 2
March	1.36	1.30	-.06	- 4
Totals	47.12	46.83	-.29	-0.6

Recognizing well-known uncertainties attending the collection of rainfall, the conclusions to be drawn from these experiments may be stated as follows: (1) Increasing the 3-inch depth of the funnel of the 12-inch gage did not increase the amount of rainfall collected; (2) increasing the 2 1/4-inch depth of the funnel of the 8-inch gage increased the amount collected by a little more than one per cent, a value within the limits of error; (3) there is no sufficient warrant in the showing made for correction of existing records made with either of the two gages, nor for recommending any change in United States

Weather Bureau gages; (4) in the design of a new pattern rain-gage the depth of funnel should be somewhat greater than 2 1/4 inches but need not exceed 3 inches.

It is known that a shallow-funnel gage was discontinued many years ago in favor of the present form, but I have not been able to locate any record of experiments made at that time.

U. S. Weather Bureau,
Washington, D. C.

WHAT ARE THE INDISPENSIBLE METEOROLOGICAL DATA FOR INDICATING THE CLIMATOLOGICAL CHARACTER OF A REGION?

A. J. Henry

The remarks I am about to offer are predicated upon Resolution No. 8 adopted at the Prague meeting of the Union in September 1927. The resolution follows:

> The Section approves the proposal of M. Eredia with reference to the usefulness of considering what meteorological data are indispensible in indicating synthetically the climatological character of a given locality and requests M. Eredia to present a complete report on this matter at the next assembly.

The essential meteorological data for determining the climatological character of a region, which M. Eredia is asked to present at the Stockholm meeting of the Union, were determined 47 years since when in 1883 that master-student of climatology, the late Dr. Julius Hann, first issued his Handbuch der Klimatologie.

This work has gone through three editions, the last being in 1908. It is recognized that there is need of a revision of Hann's work. For a year or more arrangements have been perfected for the preparation of a new Handbook of Climatology under the editorship of Dr. W. Koppen, who for many years was associated with the Deutsche Seewarte of Hamburg. The material for this work is being assembled and divisional editors have been chosen.

It is difficult therefore to escape the conclusion that the resolution adopted at Prague in 1927, if further prosecuted, will conflict with work already organized and progressing in an orderly manner looking to the same end; moreover, at the meeting of the International Meteorological Organization at Copenhagen in September 1929 a Climatological Commission was created under the presidency of M. Von Ficker (Proces Verbaux des Seances de la Conference International des Directeurs Comite Meteorologique International, p. 77).

In view of the facts hereinbefore set out, it would seem that further action on M. Eredia's proposal should not be taken and it is so recommended.

U. S. Weather Bureau,
Washington, D. C.

DAILY SYNOPTIC WEATHER CHARTS OF THE NORTHERN HEMISPHERE

A. J. Henry

The General Assembly of the Union at the Madrid meeting in 1924 and at Prague in 1927 adopted certain resolutions with reference to the preparation of daily weather-charts of the Northern Hemisphere. Inasmuch as the subject will doubtless come up for action at the Stockholm meeting, a review of previous action may be helpful.

Resolution No. 26 adopted at Madrid is as follows:

> The Section decides to make, as an example and on a precisely limited subject, a test of the services which would be rendered by the International Meteorological Bureau, the organization of which is now being studied by a commission of the International Meteorological Committee.
>
> The problem to be solved, chosen from those which clearly involve international collaboration, is the following:
>
> The compilation of an atlas of daily or bidaily charts covering the greatest possible part of the Northern Hemisphere for the third quarter of the year 1923.
>
> M. La Cour, president of the North Atlantic Chart Commission, will have charge of the execution of the work. He will work under the instruction of a commission including members of the Union belonging to the above commission of the International Committee. A sum of £500 will be allotted for the work.

At the General Assembly in Prague in 1927, Resolutions Nos. 4 and 15 bearing upon the subject of compiling daily weather charts for the Northern and Southern Hemispheres were adopted. No. 4 is as follows:

> (4) In consideration of the progress accomplished by the network of observational stations in the Pacific Ocean the Section considers it necessary that the experiment of publishing (in conformity with Resolution No. 26 of the Assembly of Madrid which relates only to the Atlantic Ocean) daily synoptic charts be completed by extending them to include the entire Northern Hemisphere for the months of August, September, and October 1927. The Section urgently requests the central bureaus of the different countries to assist the editor in this undertaking by furnishing him the necessary data duly verified and in the form that will be proposed by him.

The purpose of Resolution No. 4 of the Prague meeting was to extend the scope of Resolution No. 26 of Madrid by extending the daily synoptic charts for the Atlantic Ocean to include the entire Northern Hemisphere, but for a slightly different set of months and for 1927 instead of 1923.

It should be noted, however, that while the Madrid resolution specifically provided for cooperation with the commission of the International Meteorological Committee, no mention of cooperation is mentioned in the Prague resolution. The Prague meeting passed a second resolution with reference to the preparation of daily weather charts for the Southern Hemisphere and set aside the sum of £100 sterling for the expenses of preparation and publication.

Without commenting upon the wisdom of preparing two sets of weather-charts for the Northern Hemisphere, both for summer months, I pass to consideration of

the fact that for a year or more, as many as four or five of the greater meteorological services of the Northern Hemisphere have prepared, in some cases daily, and in others bidaily, weather-charts of the Northern Hemisphere; moreover, the London Meteorological Office not only has prepared such charts but has printed them on the date of issue. These charts are confessedly incomplete owing to the fact that almost a quarter of the Northern Hemisphere does not possess a network of observing stations sufficiently close to permit the preparation of a complete and reliable chart.

Further comment upon the consideration of resolutions with respect to preparing daily weather-charts of the Northern Hemisphere seems to be unnecessary.

U. S. Weather Bureau,
Washington, D. C.

REPORT ON BULLETIN ON METEOROLOGY IN THE SERIES "PHYSICS OF THE EARTH" ISSUED BY THE NATIONAL RESEARCH COUNCIL

H. H. Kimball

Manuscript for this Bulletin was prepared under the direction of a committee named by the chairman of the Division of Physical Sciences, National Research Council, consisting of the following: Willis R. Gregg, Chief of Aerological Division, Weather Bureau; Alfred J. Henry, Editor Monthly Weather Review, Weather Bureau; Wm. J. Humphreys, Meteorological Physics, Weather Bureau; Herbert H. Kimball, in charge Solar-Radiation Investigations, Weather Bureau; Charles F. Marvin, Chief of Weather Bureau; Carl-Gustaf Rossby, Associate Professor Meteorology, Massachusetts Institute of Technology; Richard Hanson Weightman, Forecaster, Weather Bureau; Hurd C. Willett, Assistant Professor of Meteorology, Massachusetts Institute of Technology; Edgar W. Woolard, Assistant Professor of Mathematics, George Washington University.

The manuscript of this Bulletin, just completed, contains about 360 pages, including 9 tables of data, but exclusive of the Table of Contents. There are 100 illustrations, mostly line drawings.

Chapter 1, by Professor Humphreys, treats of the origin and the composition of the atmosphere. It is pointed out that when the material composing the Earth was in some way separated from the Sun it included all the elements contained in that body. From these, through cooling, the solid Earth, bodies of water, and the gaseous atmosphere have been formed.

Many ages passed before the Greeks discovered that evaporation of water from the Earth's surface and its condensation into clouds in the atmosphere caused rainfall. In spite of this discovery, until 2,000 years later (1750 A.D.) the belief was still held that air was one of the four primary elements of which earth, water, and fire were the other three. Then, with the development of the science of chemistry, carbon dioxide, nitrogen, and oxygen, and in recent times the lesser gases, were discovered. The author gives dates of discovery and relative amounts of the several gases.

Chapter 2, by Professor Henry, treats of meteorological data and meteorological changes. Two periods are considered. In the first, the non-instrumental, it is necessary to rely on geologic records, among the most important of which are those left by glaciers. Connecting the geological past with the present are rings of tree-growth in petrified forests and in living trees. Also,

there are evidences of pulses in rainfall for centuries past, as recorded by the advance and retreat of the shores of certain lakes. It was from non-instrumental records that Bruckner established the cycle of meteorological events that bears his name.

The second, or instrumental period, began with the invention of instruments for measuring air pressure, temperature, humidity and movement, the precipitation from the atmosphere, and the movement of clouds, together with a classification of clouds that permitted them to be systematically recorded. The science of meteorology grew out of a study of these instrumental records. The science was popularized by the discovery that when properly charted meteorological observations made it possible to locate and predict the future movement of storms and the weather-changes that accompany them. Meteorological observing-stations multiplied to such an extent that it has been impossible to give to the data accumulated the study they deserve. Several summaries of meteorological data that are helpful in such studies are described briefly.

Chapter 3, by Dr. Kimball, treats of solar radiation and its role. Instruments employed in solar-radiation measurements are described. Solar spectrum energy-curves are reproduced, and the effect upon energy of different wave-lengths in passing through different amounts of pure dry air, water-vapor, and dust is shown, a distinction being made between selective absorption and general scattering.

A table gives the relative amount of energy included in different parts of the spectrum after it has passed through atmospheric paths of different length and containing different amounts of water-vapor and dust. The intensity of that part of the spectrum that gives to the human eye the sensation of light, and of the short-wave or ultraviolet radiation, receive special attention. It is shown that in great industrial centers, unless special precautions are taken to mitigate the smoke-nuisance, the so-called health-rays in the ultraviolet will be almost completely eliminated in mid-winter. Also, the intensity of daylight, or natural light, is greatly diminished.

Solar radiation affords the energy necessary to keep the atmospheric engine running, and therefore is the primary cause of diurnal and seasonal weather-changes. A very small percentage of the total energy available is directly utilized to stimulate plant-growth. So enormous is this supply of energy that its utilization as a source of power has been investigated, but thus far without showing that it can be applied economically.

Chapter 4, by W. R. Gregg, Leroy T. Samuels, and Welby R. Stevens, describes quite fully instruments and methods employed in exploring the upper air for the purpose of obtaining meteorological data. The results obtained are generally presented in the form of graphs. While special attention is devoted to data obtained in the United States, these are also charted in such a way as to show their relation to upper-air conditions in other parts of the world.

In general, at altitudes sufficient to eliminate marine and continental effects, the meteorological changes are attributable to changes with latitude or with elevation. For example, while in England in winter surface-temperatures are markedly higher than at Ellendale, North Dakota, the northernmost aerological station in the United States, at an elevation of a few kilometers these temperature-differences almost completely disappear.

Averages of air temperature, pressure, density, humidity, and wind-direction and wind-velocity are given for different months or seasons, and for differ-

ent altitudes, as determined by observations at the different aerological stations in the United States east of the Rocky Mountains.

Chapter 5, on dynamic meteorology, written by Professor Willett, is a marked departure from the preceding chapters. As its name implies, it treats of the energy, principally derived from solar radiation, that is available for keeping in motion what has been so aptly termed the atmospheric engine. The subject is discussed under seven principal divisions, as follows: (I) The gas-laws as applied to the atmosphere; (II) the vertical distribution of pressure and temperature in the atmosphere and the effect of vertical displacements of such distributions; (III) effect of vertical movements in the atmosphere on the temperature lapse-rate; (IV) radiation; (V) the dynamics of air-currents; (VI) the general circulation; (VII) secondary circulation, cyclones and anti-cyclones.

The author has attempted, very successfully, to give a summary of the results of recent studies, mostly European, in meteorological science. He does not attempt to give a final solution to meteorological problems, but rather to show the present trend of meteorological investigation. Of necessity, certain fundamental laws have been developed mathematically, but in much of the chapter processes and phenomena have been described without the use of mathematics.

Chapter 6 by R. H. Weightman treats of the physical basis of weather-forecasting. The bearing that some of the laws outlined in Chapter 5 have in predicting the weather is shown. Others are difficult of application even in a qualitative sense. While there is available a considerable amount of current information on the wind-velocities and wind-directions aloft, there are comparatively few observations of temperatures and humidities. The time available for analyzing conditions depicted on a weather-map is not sufficient to apply mathematical equations even if all the terms that enter into the equations were known. In weather-forecasting few physical laws can be applied quantitatively; some, however, are susceptible of a qualitative interpretation that is of considerable value. Forecasting still remains largely a matter of judgment born of long experience and study of the weather-maps.

At the end of each chapter references are given that will enable the interested reader to pursue further the subject discussed.

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Washington, D. C.

SECTION OF TERRESTRIAL MAGNETISM AND ELECTRICITY

(Washington, D. C., May 2, 1930)

PROGRAM OF THE INTERNATIONAL POLAR YEAR 1932-1933

H. U. Sverdrup

In 1875 Lieutenant Karl Weyprecht of Austria brought forward the definite plan for an International Polar Year. He maintained that the scientific value of polar expeditions would be greatly increased if they were arranged on a uniform plan to give simultaneous physical observations over a full year at a number of places in high latitudes. Weyprecht's plan was realized in the year 1882-83. From August 1882 to August 1883, fourteen expeditions, which were sent out by twelve different countries, were in the field--twelve in the arctic and two in the antarctic, all fully equipped for comprehensive meteorological and magnetic observations.

As to the scientific and practical value of the results of the first International Polar Year, attention may be called only to the fact that the present magnetic charts north of the arctic circle are based to a great extent upon the observations which were taken in 1882-83, and that the meteorological charts for that year have formed the basis of many notable studies in weather-forecasting.

In the long period which has elapsed since the first Polar Year many new problems have arisen requiring additional data for their solution while the instruments and methods for observing have been greatly improved.

The establishment of international cooperation for the period of one year in order to obtain new data from high latitudes, therefore, seems highly desirable. About two years ago a definite proposal pertaining to arrangement of a new Polar Year in 1932-33 was submitted to the International Committee of Meteorological Directors through its Commission for the Reseau Mondial and its Commission for Terrestrial Magnetism and Atmospheric Electricity. These Commissions, acting upon this proposal, appointed a joint sub-commission consisting of Dr. la Cour of Copenhagen, Dr. Simpson of London, Dr. Hergesell of Lindenberg, Captain Wöhrle of Paris, and Dr. Sverdrup of Bergen. The program prepared by this sub-commission was discussed at the meeting of the above-mentioned Commissions in Copenhagen in September 1929, and the following resolutions were adopted:

> (9) The Conference is of the opinion that magnetic, auroral, and meteorological observations at a network of stations in the arctic and antarctic would materially advance present knowledge and understanding of the magnetic, auroral, and meteorological phenomena not only within the polar regions but in general. The Conference is also of the opinion that this increased knowledge will be of practical application to problems connected with terrestrial magnetism, marine and aerial navigation, wireless telegraphy, and weather-forecasting.
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> (10) The Conference is of the opinion that such observations should be carried out for one whole year and that international cooperation is necessary to ensure this.

(11) The Conference proposes that such international cooperation shall be established during the year 1932-33, which is the Jubilee Year of the first International Polar Year in 1882-83.

(12) The Conference has received a report of a sub-commission appointed by the Commission for the Réseau Mondial and Polar Meteorology and the Commission for Terrestrial Magnetism and Atmospheric Electricity to consider the question of the proposed Polar Year, and is of the opinion that the scheme there outlined forms a suitable basis for the scientific work to be undertaken.

(13) The Conference appoints a new Commission to be entitled "The Commission for the Polar Year 1932-33," to be charged with the carrying out of the undertaking. This Commission will prepare detailed plans of the observations to be made and the methods of making them and will take all steps possible to coordinate the work of the various countries taking part so that the greatest value may be obtained from the scientific work carried out during the Polar Year.

(14) The Conference appoints: The President, Commission for the Réseau Mondial and Polar Meteorology; the President, Commission for Terrestrial Magnetism and Atmospheric Electricity; the President, Commission for the Investigation of the Upper Air; Dr. D. la Cour (Denmark); M. Karpinsky (U.S.S.R.); M. J. Patterson (Canada); and Dr. H. U. Sverdrup (Norway) to be members of the Commission with power to add to their number representatives of countries which express their intention to take an active part in the Polar Year.

(15) The Conference instructs the Bureau of the International Meteorological Organization to bring the scheme for the Polar Year to the notice of all Governments in order to obtain the international cooperation on which the scheme depends.

(16) The Bureau is also instructed to issue an invitation to the International Union of Geodesy and Geophysics to support the undertaking and to appoint representatives to cooperate with the "Commission for the Polar Year 1932-33.

The situation at the present time is that the "Commission for the Polar Year 1932-33" is preparing detailed plans of observations to be made and the methods of making them. The different memoranda will be completed before the meeting of the International Geodetic and Geophysical Union at Stockholm in August 1930. It would have been very desirable if these memoranda were available at this time, making it possible for the National Committees to discuss them in detail. However, it is possible now to present some information which will give a rather definite idea of the proposed scientific work.

In preparing the first proposals which were submitted at Copenhagen the sub-commission followed some general principles definitely fixing a minimum program for each station. Thus the subjects of investigation should be such as at present can be well defined for simultaneous observations at a network of stations and the personnel necessary should be small in order to keep the expense of operation low.

The sub-commission recommended that the activity during the Polar Year 1932-33 should concentrate upon securing data to advance the study of the following subjects: (1) Magnetic secular-changes since 1882-83; (2) completion

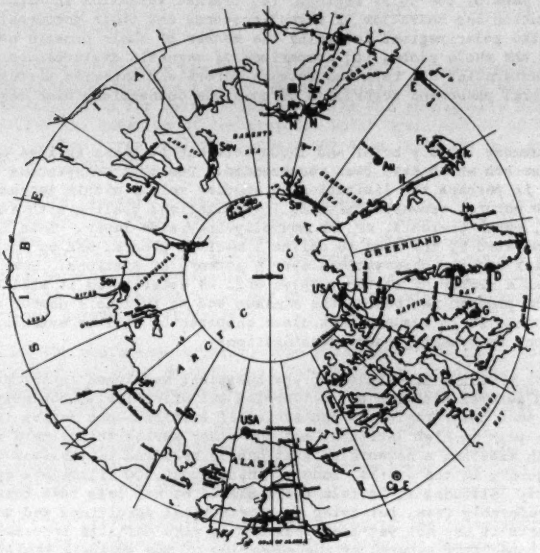
of magnetic maps of the polar regions; (3) diurnal variation in high latitudes; (4) distribution and character of magnetic storms and their perturbations throughout the polar regions including the manner of their passage over these regions and the whole globe; (5) comparison of magnetic disturbances in the arctic and antarctic; (6) relation between aurora and magnetic disturbances; and (7) auroral phenomena regardless of possible connections with magnetic conditions.

This summary is very brief and incomplete but it gives an idea of the lines of research which have been recommended. The most conspicuous feature of the program is perhaps the limitation to studies mainly within terrestrial-magnetic and auroral research omitting investigations dealing with atmospheric electricity, earth-currents, or the Kennelly-Heaviside layer. This limitation has been indicated by the need to define a minimum program and by that to select subjects which require observations over a network of stations. However, it is hoped that at a number of stations which will be established it will be possible to extend the program far beyond the minimum and to include a number of special investigations. It is assumed that close cooperation will be established between stations undertaking such investigations.

In order that the discussion of the questions mentioned in terrestrial-magnetic and auroral research may be carried out effectively, observations are required in each polar region at two series of stations--one series to encircle the magnetic pole in high latitudes and the other series to follow a magnetic meridian. In addition a network of stations is required in the zone of maximum auroral frequency in the arctic, some of these about 100 kilometers apart. In high southerly latitudes it is felt there should be not less than three stations and preferably five, but owing to geographical conditions and the difficulty of access it has not yet been possible to make definite proposals. However, it is hoped that several of the countries of the southern hemisphere may wish to take part in the antarctic work and also that some countries of the northern hemisphere will find it possible to fit out small expeditions for which whalers voyaging in the antarctic seas may be willing to supply transport. For the arctic more definite proposals can be made. There are already six permanent magnetic observatories above 60° north latitude and three of these are on or near the sites of stations occupied in the first Polar Year. It is important that the other stations of 1882-83 be reoccupied in 1932-33. The accompanying map indicates these stations and the countries which occupied them as well as the new stations proposed to fill out the gaps in the network. In selecting the new stations, consideration has been given the problems to be studied and the possibility of easy transport. They have been assigned tentatively to definite countries and it is hoped that these countries will find it possible to establish them.

All the magnetic stations should be equipped with automatic recording instruments. The sub-commission recommends that three sets of recording instruments be installed at each station--one of ordinary sensitivity, one of low sensitivity, and one for quick runs. The routine work at the magnetic stations requires efficient control of the recording instruments including observations for absolute determinations.

As to the auroral observations it is deemed desirable that every magnetic station be supplied with special cameras for photographing auroral displays. Simultaneous photographs from two points to determine the altitude of the aurora should be obtained at a small number of stations only. In addition to the photographic work, eye-observations of the aurora will be of great value. It is hoped that not only magnetic stations will take part in this work but that



Polar chart above latitude 60° north, showing existing observatories, stations of 1882-83, and additional proposed new stations 1932-33

the existing meteorological stations also will assist. Such eye-observations should be undertaken at the same Greenwich time at all stations. A pocket spectroscope is being designed by which presence or absence of aurora during daylight or when the sky is overcast can be determined as well as possibly an approximate measure for auroral intensity.

A personnel of three men, one a magnetician experienced in taking magnetic observations in high latitudes, should be sufficient for those magnetic stations executing the minimum program. The Observatory at Sodankylä (Finland) is willing to receive visitors for instruction in magnetic observations under arctic conditions. The new observatory for auroral and magnetic studies, recently built in Tromsö (Norway) through the generosity of the International Education Board, is ready also to train observers in the methods followed there in auroral observations.

As regards details of the meteorological program proposed, efforts will be made to establish a number of high-level stations within the arctic circle and a few stations for aerological researches by means of sounding-balloons. Ordinary meteorological stations exist now at or near most of the localities occupied in 1882-83 and a special program for pilot-balloon work will be suggested for them.

The proposals for the Polar Year 1932-33 have been received favorably by the scientists and scientific institutions in a number of countries. The time which has elapsed since the meeting in Copenhagen has been too short to secure replies from the governments which have been asked to take part but already Denmark, Germany, Norway, the Soviet Republics, and Spain have appointed committees to plan participation in the general scheme.

The proposed distribution of stations (see map) places by far the greatest number in what may be called the Atlantic Sector, because of easy accessibility to high northerly latitudes within this area. Since this area has greatest interest to Europe, it has been proposed that stations therein be occupied by European nations. However, that part of the program to provide a series of stations encircling the pole is regarded as equally essential to the success of the enterprise as is the establishment of the dense network of stations in the Atlantic Sector. The Soviet representatives have already intimated that their government will not only reoccupy the stations of 1882-83 but will establish two or three new intermediate stations. The stations of the first Polar Year from Greenland to Bering Strait were occupied by the United States and Canada except Kinguafiord, which was maintained by Germany. The stations of the United States at Lady Franklin Bay and at Point Barrow were of the greatest importance. Reoccupation of Point Barrow is especially desirable although Lady Franklin Bay may perhaps be omitted because Denmark intends to establish a station at Cape York in Greenland. On the other hand, it is doubtful whether Germany will be able to reoccupy Kinguafiord and for this reason it may be desirable that provision for this station be made by some organization in Canada or in the United States. It is hardly necessary to emphasize the importance of the cooperation of the United States and the value of any step taken in this country towards extending cooperation beyond the limits suggested in the proposals for the Jubilee Polar Year.

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PRACTICAL IMPORTANCE OF CONTINUING MAGNETIC OBSERVATIONS ON THE OCEANS

G. W. Littlehales

It is well known in the Section of Terrestrial Magnetism and Electricity of the American Geophysical Union that America has been endeavoring to pay back to the world her share of the debt which human welfare owes for the magnetic compass and especially to Europeans for the often-acknowledged benefits that have been derived from the labors of their great mathematicians in the measurement and application of magnetic forces by giving in return comprehensive surveys of terrestrial magnetism to provide scientists with facts for the future molding of philosophic thought, and captains of industry with more perfect means for the adaptation of geophysical agencies to the working power of commerce.

The potent influences of terrestrial magnetism reach at least as far back as the beginnings of oceanic navigation, and the empire of our civilization is built around it as far as that has been shaped by oceanic transit and oceanic commerce.

From the magnetic surveys that have been made, mathematical expression may now be given, for the first time in history, of the magnetic state of the

Earth at about the present epoch; but, in order to reap the fruits of the life-blood and the treasure spent in reaching this stage of progress, there must now be supplied the differentials of the mathematical expression. To do this a continuance of the taking of magnetic observations by revisiting many former points of observation on the oceans, as well as on the land, is essential.

Research points to the conclusion that the Earth is alive with ever-changing magnetic forces, and that the magnetic state at any certain place is not only altering in a slow, progressive manner, like the march of the constellations in the heavens, but that it has its daily tides like the ocean, and also like the sea is traversed by waves of changing magnetism or stirred into magnetic storm; and, in the present state of knowledge, the causes of all these changes are associated with an indefinable something, which, like an undetected planet, we recognize by its workings, although as yet unable adequately to describe. There is no doubt that great benefits to the world are vouchsafed by conferring on mankind an understanding of terrestrial magnetism.

(To reflect the importance and indeed the indispensableness of continuing magnetic observations especially on the oceans, a chart of the world was exhibited showing by diagrams applying to different regions the course of the movement of the magnetic needle through the centuries and the great changes which time works in its pointing, upon which shipping depends for guidance upon the sea.)

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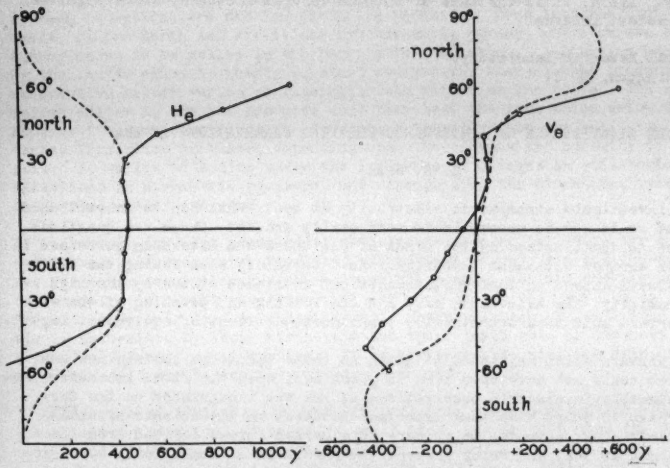
SOME SUGGESTIONS FOR AURORAL AND MAGNETIC OBSERVATIONS IN POLAR REGIONS

E. O. Hulburt

The frequency of the occurrence of aurorae has been found to increase rapidly with the magnetic latitude of the observing station, reaching a maximum zone about 23° from the north, or south, magnetic pole and decreasing at higher latitudes. Records from antarctic expeditions, notably those of Sir Douglas Mawson, show that strong auroral displays are usually most frequent in the evening hours before midnight at all latitudes above 60° south, that the frequency of the quiet aurorae are a maximum before midnight for magnetic latitudes below 67°, and that the maximum shifts to the early morning hours at magnetic latitudes above 67°; at certain stations in the very high latitudes there were two maxima, one in the evening and one in the morning. The records from the arctic regions, which are less complete than those from the antarctic, show much the same diurnal distribution of the aurorae. In general it seems that the geographical distribution of the various types of aurorae, as arches, glows, streamers, etc., is roughly known for latitudes above the zone of maximum auroral frequency, but is not so well known at lower latitudes. Detailed observations in the lower latitudes, as well as at the higher latitudes, might be of value. If such records are obtained it is to be hoped that they will be published in full. A complete report would be of immeasurably greater value to the development of a theoretical explanation than a summary or an abbreviated outline.

The auroral records of a single station on a parallel of latitude are probably not entirely representative of the auroral phenomena of the parallel, for two stations at different points on the same parallel sometimes observe differ-



Figs. 1 and 2--Observed and theoretical values of H_e and V_e

ent auroral features. Winds and weather, etc., in the high atmosphere may be as important in the explanation of the aurora as the influx of energy in the form of ionization.

The curves of the diurnal variations of the Earth's magnetic field during magnetic storms undergo a marked change in their characteristics at the zone of maximum auroral frequency. The magnetic variations are thought to be due to changes in the ionization of the upper atmosphere. Measurements by means of pulses of wireless waves of the density of the ionization and of the height above the Earth at various latitudes in polar regions during magnetic and auroral calms and disturbances would be of great interest and value. The wireless-wave methods offer a means of plumbing the outer depths of the atmosphere by direct experiment, and thus might be expected to yield information obtainable in no other way at the present time.

L. A. Bauer, in his 1922 analysis of the Earth's magnetic field, showed that about six per cent of the field was of external origin. The horizontal and vertical components H_e and V_e, respectively, of the portion of the field of external origin are given by the heavy line curves of Figures 1 and 2. It is seen that the analysis extended only to 60° latitudes. A recent theory showed that the ionization in the upper atmosphere would cause an electrical current flowing around the Earth mainly in the tropical latitudes. The theoretical values of H_e and V_e arising from the current are given by the dotted curves of Figures 1 and 2. The observed and theoretical curves agree fairly well, with some discrepancies. One wonders whether the discrepancies are real--if so, something has been left out of the theory--or whether they will disappear upon further attention to observation. It would be of interest to gather magnetic data in polar regions for the purpose of extending the analysis to higher

latitudes. Again, it is the zone of maximum auroral frequency which might be of the greatest interest.

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THE SIGNIFICANCE OF ATMOSPHERIC-ELECTRIC OBSERVATIONS AT SEA

O. H. Gish

Why investigate atmospheric electricity at sea? What can be accomplished there that could not be accomplished more easily ashore? These are questions which have no doubt arisen in the minds of all those who have been concerned in one way or another with such investigations. Certainly when facing the difficulites always attending such measurements but increased at sea by the high average humidity, the salt-laden air, and the rolling and pitching of the vessel, observers must have emphatically propounded questions of equivalent import.

The answers which may today be given to these questions contain new aspects which could not have been seen 15 years ago, when the first intensive program of atmospheric-electric observations at sea was inaugurated on the Carnegie. These 15 years have seen important advances in the science of atmospheric electricity, advances which were to a marked degree derived from the data obtained at sea, and which present the phenomena of atmospheric electricity in a new and improved perspective. The answers to our questions and the full significance of atmospheric-electric observations at sea will be seen best if viewed in this perspective. It may assist in this respect to review briefly some aspects of the science. It will be recalled that in so far as concerns atmospheric-electric phenomena the Earth may be considered a large electrically conducting sphere, charged with negative electricity and surrounded by an atmosphere which to a small degree conducts electricity due to the presence there of electric carriers, or ions--some positive and some negative. The positive ions are attracted to the Earth and tend to neutralize its negative charge, while those of negative sign are repelled. Thus neither the charge of the Earth nor the conductivity of the atmosphere could continue long without some source of replenishment for each of them. Calculations show that the charge would fall to one-tenth its initial value in 12 minutes if the conductivity as observed remained undiminished.

The sources of replenishment of the ions in the air are such ionizing agents as the radioactive matter of the Earth and of the atmosphere, together with the penetrating radiation. The charge of the Earth is maintained by some elusive, unknown factor, which we may call the compensating current. To ascertain the origin and character of this compensating current constitutes one of the most important objectives in present-day atmospheric-electric investigations

Although the Earth's charge or its measure--the potential gradient at the surface--and the conductivity of the air, are both on the average constant, yet interesting variations occur in both these features. The conductivity which depends upon both the number of small ions and upon their mobility will be affected by variations in these elements. The variations in the mobility of these ions is, however, of only minor importance. It is the number of ions (small mobile ions) which is the controlling factor in conductivity. This number is dependent both upon the rate at which ions are formed and upon their rate of decay or diminution. As already mentioned, the radiations from the

radioactive matter in the Earth and the atmosphere, together with the penetrating radiation, are the ion-producing agencies. Over land the radioactive matter of the Earth and air is the predominating agency, while at sea the only agency known to be active is the penetrating radiation. Ionization produced by the latter cause is nearly constant everywhere, whereas that produced by radioactive matter varies considerably both with time and place. The radioactive matter in the air produces more than half the ions which arise from radioactive sources over land. It comes originally from the soil, and hence varies from place to place, depending upon the content and porosity of the soil. It varies with time under the influence of changes in soil-temperature, variations in barometric pressure, and depends upon the direction, velocity, and turbulence of the wind.

Although ions are produced at the average rate of ten pairs per cubic centimeter per second over land, and at the rate of only one and one-half to two per cubic centimeter per second over the oceans, yet the average number of ion-pairs in a cubic centimeter of air near the Earth's surface is roughly 700, not only over land but also at sea. The population of small ions in the air is determined by their birth-rate and their death-rate or the rate of formation and rate of diminution. It is apparent then that the rate of diminution over land is considerably greater than over sea.

The rate of diminution depends in part upon the recombination which takes place between positive and negative small ions, and in part upon the union of these small ions both with large, relatively immobile, ions and with certain electrically neutral particles or molecular complexes. These large ions and molecular complexes also serve as nuclei, or starters, in the condensation of water-vapor. On this account they are called condensation-nuclei. Everywhere in the lower kilometer of the atmosphere these nuclei occur in sufficient abundance to constitute the element which chiefly determines the rate of diminution of small ions. Their number and corresponding effect in decreasing the average life of small ions is much greater over land than at sea and is there subject to large fluctuations which, especially in the vicinity of cities, often arise from man-made conditions.

The conductivity of the atmosphere thus affected has in turn an influence upon the Earth's charge and upon its attendant electric force, or potential gradient. Thus, for example, the low and fluctuating conductivity in the vicinity of cities gives rise to high and fluctuating values of the potential gradient. However, the Earth's charge as measured by the potential gradient varies from other causes. Thus, the electric charges generally connected with dust, smoke, fog, etc.; and especially those intense charges which are developed in thunderstorms, all affect the potential gradient directly. Effects arising from these causes will,obviously vary in an irregular manner from place to place and from time to time, especially over land.

There is also another source of variation in the potential gradient, to which attention is especially invited. From a study of the potential-gradient data obtained on cruises IV, V, and VI of the Carnegie, Mauchly was able to conclude that the regular change in this element during the day over the ocean proceeds on a universal schedule. Thus, for example, high values tend to occur everywhere at about 18 to 20 hours (6 to 8 p.m.) in Greenwich meridian time. He also later found evidence, which supports the view, that the same phenomenon occurs over land. However, other effects such as have already been mentioned, and which are of very local origin, are there entangled with the general world-wide effect. We thus see here evidence of a phenomenon world-wide in extent and simultaneous in occurrence, the manifestation of a factor

which affects the entire Earth at the same instant of time and which waxes and wanes in a regular manner throughout the day. This factor is likely the compensating current. That it was possible to make a discovery of such far-reaching importance on the basis of the relatively small amount of data available at the time gives an intimation of the feasibility of atmospheric-electric measurements at sea and emphasizes the advantage of studying atmospheric-electric phenomena under the simple conditions which prevail over the oceans. The greater uniformity and simplicity in the circumstances which affect atmospheric-electric conditions at sea necessitate fewer data in a given position and thus with a moving observatory it is possible to obtain a wide distribution of observations over the great ocean-areas of the Earth, a point of no small importance to the study of a world-problem.

The relative uniformity of conditions at sea have been most vividly brought out by the photographic registrations obtained on Cruise VII of the Carnegie. It will therefore be interesting to examine some sample records and to briefly describe the instruments used in obtaining them. The instrument used for recording the air-potential is housed in a case with a water-tight cover; the latter may be removed during a test of the insulation. The electroscope used here and in all the atmospheric-electric work on board the Carnegie employs, instead of the gold leaves often used, fine fibers spun from quartz and coated with platinum. These are of the thickness of fine spider-threads. They are attached at one end to a delicate spring, also of fine quartz. This arrangement because of its small inertia proves very satisfactory for measurements on a moving vessel. In the recorders the shadow of the quartz-fiber is cast upon a sheet of photographic paper which is steadily moved by clock-work. A record of the air-potential obtained on the Pacific Ocean on November 11, 1929, with this apparatus is reproduced in Figure 1. The high values of potential here shown at about 18 hours Greenwich meridian time is typical of the oceans. This curve alone does not bring out the uniformity and simplicity of atmospheric-electric conditions at sea. It is only in a wider comparison of records obtained at sea and over land that this becomes apparent. However, the fact that with this recorder, records sufficiently free from irregularities to be classed as "zero-days" have been obtained on 60 per cent of the days at sea, whereas at a favorable land-observatory only 40 per cent is obtained, bears evidence of the greater uniformity and simplicity of atmospheric-electric conditions at sea.

The diurnal variation in air-potentials as derived from eye-readings obtained on previous cruises and those from the 21 records obtained in the Atlantic

Fig. 1--Sample potential-gradient record, November 11, 1928

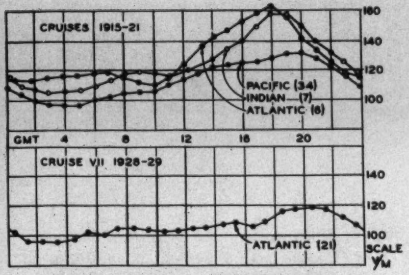

Fig. 2--Comparison of diurnal variations in atmospheric potential-gradient obtained in the Atlantic (21 sets) with previous results

Ocean on Cruise VII are compared in Figure 2. It will be seen, as mentioned before, that the recent observations bear out the remarkable discovery that the regular change in air-potential during the day proceeds on a universal-time schedule.

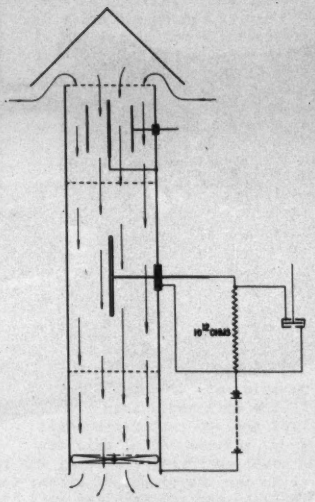

Fig. 3--Diagrammatical representation of method of measuring air-conductivity

The general features of the method employed for recording the conductivity of air both at the observatories of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington and on the Carnegie are shown diagrammatically in Figure 3. Air is drawn through a large tube in the manner indicated by the arrows. It passes around a small cylinder which is supported at the center of the large tube. A difference of potential is maintained between the small cylinder and the large tube by means of a battery. A current will then flow through the air from the cylinder to the walls of the air-flow tube and, of course, this will be greater the greater the conductivity of the air. This current must also flow through the very high resistance marked "10^{12} ohms," which signifies one million megohms. Although this current is extremely small (less than a millionth of a millionth of an ampere) there will result a difference of potential of about one volt between the terminals of the high resistance. This acts upon an electrometer and, in the instrument used on the Carnegie, produces in the sensitive quarts-fibers, deflections which are proportional to the conductivity of the air that flows through the tube. The proportionality-factor is determined by means of a device which supplies small currents of known value. In the upper part of the large tube are placed two concentric cylinders, so arranged that a high potential may be applied between them for a few minutes once each hour. This removes all the ions from the incoming air so that the record for this period will correspond to zero-conductivity and gives a starting point from which to measure values recorded at other times. The actual recorder as it appears in action is shown in Figure 4. The electrometer, the high resistance, and other parts which may be detrimentally affected by moist and salt-laden air are enclosed in the tight metal-box seen at the left of the figure. Calcium chloride is placed in this case to absorb the moisture which may slowly

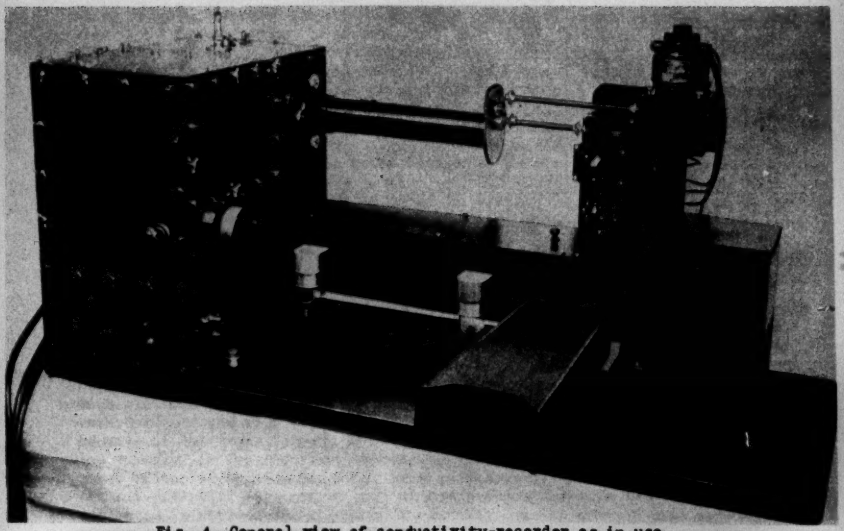

Fig. 4--General view of conductivity-recorder as in use

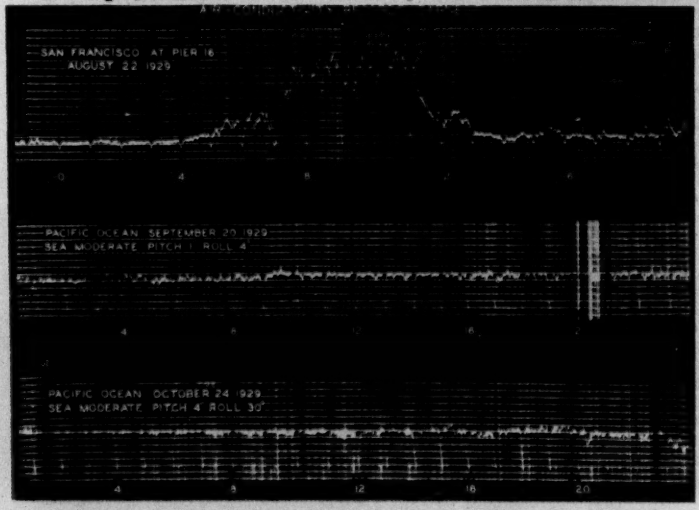
SAN FRANCISCO AT PIER 16
AUGUST 22 1929
PACIFIC OCEAN SEPTEMBER 20 1929
SEA MODERATE PITCH 1 ROLL 4
PACIFIC OCEAN OCTOBER 24 1929
SEA MODERATE PITCH 4 ROLL 30

Fig. 5--Sample conductivity-records obtained on Carnegie--(a) at San Francisco pier, August 22, 1929; (b) average record at sea, September 20, 1929, and (c) most disturbed record at sea, October 24, 1929

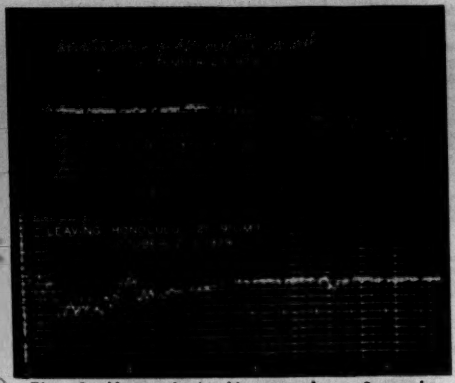

Fig. 6--Air-conductivity records on Carnegie--approaching Honolulu, September 23, 1929, and leaving Honolulu, October 2-3, 1929

gain an entrance. The paper on which the record is made is carried in the light-tight drum seen on the right. The mechanism protruding horizontally from the upper part of the case is that used in providing the small current of known value for the purpose of finding the proportionality-factor.

Samples of the records obtained with this recorder are reproduced in Figure 6. The upper one, obtained on the Carnegie while she was tied to Pier 16 in San Francisco Harbor, is an excellent example of the large fluctuations which are to be found over land. The highest values here occur from midnight until four o'clock local time in the morning, the time when the air is purest. These high values are generally less than those recorded at sea. That they appear higher in the record is due to the use of a greater sensitivity for the San Francisco record than was used for the records at sea. The zero-records can be seen at each hour. Records obtained at San Francisco throughout most of August all have this general character. Other land stations are likely to show quite different characteristics.

The middle record represents an average day at sea, and the lower, the most disturbed record, was obtained while the vessel was rolling through an angle of 30° and pitching 4°. Neither record has been retouched. The difference in the character of these and that of the land-record is striking. Another comparison which brings out this difference between the electric condition of the air over the open sea and that over land is shown in Figure 6. The upper record was obtained on the day the vessel arrived at Honolulu. At the time corresponding to the middle of this record the Carnegie had just reached the leeward of Oahu Island. The change from the relatively steady values in the early part of this record to the large fluctuations and low values of conductivity in the latter part is so different from anything recorded on the high seas that this, together with the similar change recorded in reverse order upon leaving Honolulu and shown in the lower record, must be taken as an exhibit of the change from land- to sea-conditions.

It is hoped that this endeavor to point out the importance of atmospheric-electric observations at sea will not detract from the interest in atmospheric-electric observations over land. More observations are needed over land as well as over the seas.

In conclusion, it may be of some interest to outline other aspects of the program for atmospheric-electric investigations on Cruise VII. In order to ascertain the effects of the various factors which determine the conductivity of the air at sea, measurements were also made of both positive and negative conductivity, of ionic density, of the penetrating radiation, of the radioactive-

TABLE 1—*Summary of ship's log and of atmospheric-electric, and associated meteorological observations. Carnegie Cruise VII*

Abstract of ship's log						Potential gradient			Ionic content		
Departing	From	Arriving	At	Distance	D-v series		Program	D-v series		Program
					A	*B*		*A*	*B*	
1928-9		*1928-9*		*miles*						
May 11	Newport News	Jun 9	Plymouth	3,669	..	..	16	..	..	15
Jun 18	Plymouth	Jun 22	Hamburg	614	..	..	..	..	..	..
Jul 7	Hamburg	Jul 20	Reykjavik	1,329	..	..	5	..	..	9
Jul 27	Reykjavik	Sep 16	Barbados	5,715	21[b]	17[b]	35	6	1	36
Oct 1	Barbados	Oct 11	Balboa	1,361	4	5	..	..	1	5
Oct 25	Balboa	Dec 6	Easter Island	4,788	24	7	..	1	1	10
Dec 12	Easter Island	Jan 14	Callao	3,334	24	9	..	4	..	23
Feb 5	Callao	Mar 13	Papeete	4,470	23	7	..	3	1	26
Mar 20	Papeete	Apr 1	Pago Pago	1,274	1	4	..	..	2	8
Apr 5	Pago Pago	Apr 6	Apia	80	..	..	..	..	..	..
Apr 20	Apia	May 20	Guam	3,914	15	13	..	1	1	10
May 25	Guam	Jun 7	Yokohama	1,447	5	8	..	1	..	9
Jun 24	Yokohama	Jul 28	San Francisco	4,839	26	6	..	..	2	17
Sep 3	San Francisco	Sep 23	Honolulu	2,186	11	4	..	..	1	18
Oct 2	Honolulu	Nov 18	Pago Pago	5,777	41	9	..	4	..	30
Nov 27	Pago Pago	Nov 28	Apia	80	..	..	..	..	..	..
Totals				44,877	195	89	56	20	10	216

Conductivity			Penetrating rad'n			Relative humidity			Condensation-nuclei			Evaporation	Balloon flights
D-v series		Program	D-v series		Program	D-v series		Program	D-v series		Program		
A	*B*		*A*	*B*		*A*	*B*		*A*	*B*			
..	..	20	..	..	11	..	..	19	..	..	19	..	..
..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	9	..	..	15	..	..	22	..	..	22	1	..
6	1	38	7	1	35	..	1	34	..	1	34	10	..
..	1	6	..	1	7	..	2	6	..	2	6	..	..
4	..	33	4	..	29	4	..	29	4	..	29	9	24
4	..	24	4	..	23	4	..	25	..	2	17	6	12
3	1	26	2	2	25	3	1	30	3	1	29	9	35
..	2	9	..	2	9	..	2	9	..	2	9	2	10
..	..	..	..	..	..	..	..	..	..	..	..	..	..
2	..	10	2	..	10	2	..	13	2	..	13	..	20
1	..	9	1	..	10	1	..	12	1	..	12	..	9
2	..	17	2	..	34	2	..	29	2	..	26	..	12
6[c]	12[c]	18	..	..	17	..	..	17	..	..	18	..	17
50[d]	..	..	4	..	37	4	..	50	3	1	33	1	32
..	..	..	..	..	..	..	..	..	..	..	..	..	..
78	17	219	26	6	262	20	6	295	15	9	277	38	171

[a]The series indicated in column *A* are "complete series" while those in column *B* are "incomplete series." A day on which not more than 2 consecutive hours were missing with a total of such omissions not exceeding 4 hours in the 24 is counted as "complete"; for potential gradient a "complete series" implies also one in which there was no negative potential in 24 hours.

[b]Including under *A* one and under *B* three eye-reading series, all other diurnal-variation series in potential gradient being from photographic records.

[c]Of these 6 are "complete" and 11 are "incomplete" photographic records.

[d]Of these 46 are "complete" photographic records.

content of air at sea, and of the number of condensation-nuclei. During the earlier part of the cruise the conductivity was measured by an eye-reading method, but beginning at San Francisco the apparatus which provided a continuous photographic record was used. The other measurements were all obtained with eye-reading instruments. An additional instrument for measuring penetrating radiation and of a type different from that used on the Carnegie since 1915, including the present cruise, was put into use at Hamburg. The purpose of this was to ascertain more definitely both the absolute amount of ionization which is produced in the air by this source and to ascertain whether variations occur in this factor. In addition to the regular daily program of all these measurements, series extending throughout 24 consecutive hours were made as frequently as possible for the purpose of studying the diurnal variation.

Table 1 will indicate the extent to which this program was carried out. It will be noticed that aside from over 200 complete daily programs in each of

the elements listed, and a number of diurnal-variation series incomplete because of the development of instrumental difficulties or unfavorable weather, complete diurnal-variation series ranging in number from 15 in the case of the condensation-nuclei to 26 in the case of penetrating radiation were obtained. It is also of considerable interest to note that satisfactory records of air-potentials, free from negative potential, were obtained for 194 days, and that in the interval from San Francisco to Apia complete and satisfactory records of conductivity were obtained for 56 days. The diurnal-variation series by eye-readings were obtained at about twice the rate possible on previous cruises, while the recorders yielded satisfactory data at a rate which in the case of the potential gradient was ten times and in the case of the conductivity 25 times the rate heretofore possible when using eye-reading methods.

The gratifying success of this program is in a great measure due to the interest of the Commander, J. P. Ault, and to the enthusiasm, diligence, and skill of W. C. Parkinson, senior scientific officer, who was in charge of the work on board throughout the cruise, and O. W. Torreson, executive officer, from Washington to San Francisco, and S. E. Forbush from San Francisco to Apia, who aside from their other duties assisted in aspects of this work.

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ON THE POSSIBILITY OF USING AVAILABLE VESSELS FOR DETERMINING MAGNETIC SECULAR-VARIATION

W. J. Peters

In view of the improbability of building a non-magnetic vessel, at least in the near future, it becomes important to consider the possibilities of magnetic work on vessels not especially constructed for the purpose.

The principal object in building the Carnegie was to eliminate ship-deviations so that observations could be made frequently without the necessity of computing corrections that require an immense amount of office work, that occasion delays in publication, and that are not very satisfactory after all is done.

The mathematical theory of ship-deviations assumes that the ship may be magnetic from two sources, that is, from transient magnetism induced in the soft iron of the ship by the Earth's field, and from permanent magnetism in the hard iron. It assumes that the transient magnetism corresponds instantaneously with changes in orientation of the iron or changes in the Earth's field and that the permanent magnetism is constant. Experience, however, indicates that neither assumption is strictly true. Time-changes, buffeting of the seas, and holding one course for many days in succession are factors in the problem. Their effects cannot always be controlled to the required degree of accuracy.

Since the observations already made by the Galilee and the Carnegie are fairly well distributed over large ocean-areas, the principal desiderata now in magnetic surveys at sea are secular-variation observations, which are essential for keeping the magnetic charts up to date and more so for theoretical studies. It might be noted here that the errors found in the magnetic data of charts by the Galilee and Carnegie appear to be largely the result of imperfect knowledge of secular change. Since secular stations may be rather widely spaced

over the oceans, sufficient data could be obtained, say, once a week, depending upon the progress of the cruise.

In accordance with these ideas, it is now proposed to utilize the opportunities offered by the ship of any scientific expedition provided she is not too magnetic, and to eliminate the harmonic part of the deviations due to her magnetic character by swinging her at every station and to reduce the non-harmonic part by installing the instruments at the most favorable location on board.

The practical limits admissible in future work at sea for the magnitude of the non-harmonic part of the deviations may be inferred from the parameters (see Table 1) of the Erebus, Challenger, Gazelle, Gauss, Terra Nova, and Discovery. These were all sailing vessels with auxiliary steam or steamers with auxiliary sail, having wooden hulls iron-fastened. On the Challenger and Gauss there was no iron within about 30 feet of the magnetic instruments, other than the hull fastenings.

Table 1--Parameters of some wooden vessels

Vessel	A	A	D	E	g	h	c	f	k	P	Q	R
		°	°	°						cgs	cgs	cgs
Erebus,[a] 1839-42	0.991	0.0	+0.4	small	+.027	small	+.026	small	+.003	small	small	small
Challenger,[a] 1873-76	0.999	+0.1	+0.3	0.0	.000	.000	+.008	.000	-.033	+.013	.000	-.040
Gazelle,[a] 1874-76	0.980	+0.3	+0.6	-0.1	+.013	+.009	+.021	-.007	-.021	+.008	-.003	-.002
Gauss,[a] 1901-03	1.003	+0.3	+1.2	0.0	-.005	.000	-.012	+.001	-.013	+.002	.000	-.002
Terra Nova,[b] 1913	1.000	---	---	---	+.037	----	----	----	+.009	----	----	+.007
Discovery,[c] 1904	0.978	0.0	+1.1	0.0	----	----	+.003	.000	-.022	+.003	.000	+.004
Galilee,[d] 1906-08	1.000	0.0	0.0	0.0	-.001	-.006	.000	+.001	-.008	.000	-.001	-.001

[a] Anleitung zu wissenschaftlichen Beobachtungen auf Reisen--Magnetische Beobachtungen an Bord von F. Bidlingmaier, 1905 (p. 486).

[b] British Antarctic Expedition, 1910-13--Terrestrial magnetism, 1921 (p. 447).

[c] National Antarctic Expedition, 1901-04--Physical observations, 1908 (pp. 148-149)

[d] Researches of the Department of Terrestrial Magnetism, vol. 3, 1917 (p. 91).

The mean A_D for each ship, that is, the non-harmonic part of the declination-deviations, does not exceed 0°.3 in this list of vessels. It is constant according to the mathematical theory. Practically it has been found to vary within a range of about 1°, but some part of such changes has probably been due to changes in the instrument-corrections. Experience with the declination-compasses on the Galilee has revealed possibilities of changes in the instrument-corrections alone of more than 1° (Researches, Dept. Terr. Mag., v. 3, 61, 1917). For future work it might be noted that the instrumental constants of the marine-collimator lost with the Carnegie were remarkably stable (Researches, Dept. Terr. Mag., v. 3, 187, 1917). A similar instrument might show that the ship's constant part of the declination-deviations is indeed more stable than has been found

when using the ordinary compass.

The non-harmonic part of the horizontal intensity deviations ΔH is theoretically equal to a constant fraction of the horizontal intensity H of the Earth's undisturbed field expressed by $(\lambda - 1)H$ in which λ represents the ratio of the horizontal components of the Earth's undisturbed field to that of the ship's and Earth's combined. The value of λ differs little from unity in wooden hulls. Excepting the values for the Discovery and the Gazelle, the greatest differences from unity occurs in the value 0.991 for the Erebus which would give about 3 in the third decimal place of H for the non-harmonic part of the deviations in H in a region of high H. It would seem that even a mediocre control of the changes in λ should make the values of the correction dependable at least to within a unit in the third place.

The non-harmonic part of the deviations in dip or inclination, I, is more complicated. It depends on the vertical components of the ship's magnetism at the point of observations and on the inclination. On the Challenger it amounted to 6° or 7° in some parts of her cruise. On the Gauss and Gazelle it apparently did not exceed 0.°3 to 0.°6.

To calculate the probable magnitude for any given inclination and vertical force the parameters k and R for the vertical components of soft- and hard-iron magnetization, respectively, at the instrument would have to be determined. Their combined effect can be determined very readily with a vertical-force instrument but to separate the effects of soft from hard iron which would be necessary to enable some estimate to be made of the probable values of non-harmonic part of the dip-deviations require observations in localities with widely different values of the vertical component of the Earth's field.

All of the vessels listed had steam boilers with long funnels. The funnels have usually been regarded as contributing the major position of the vertical components of the disturbance and the irregular changes in these components have been ascribed to changes in funnel temperatures. The non-harmonic part of the inclinations is given by $\Delta_I = 2 \sin I/2 \ (2 - \mu)$ and $\mu = 1 + k + R/Z$, in which R represents the vertical magnetization of hard iron and k the coefficient for soft iron magnetized by the vertical component Z of the Earth's field.

Data for modern steel hulls usually do not contain values for the vertical parameters. There are, however, published values for early English warships (Magnetism of ships and deviations of compass. Papers by Evans & Smith, 1865. Collected in one publication by Bureau of Navigation, 1867) giving μ by which values of the non-harmonic part of deviations in dip may be computed for the localities in which the given μ has been determined. These values are found to range from a few tenths of a degree up to 10°. It is inferred that steel hulls would also give values of the same order of magnitude. It is reasonably certain that magnitude of this order could not be controlled to the required degree of accuracy.

Details in the methods of observing by swinging ship, etc., at every station belong to specific instructions but it might be well to note some of the departures from former practice that would be permissible. As it is proposed to eliminate the harmonic part of deviations by swinging, it is not necessary to determine the coefficients. Four headings are sufficient to eliminate the harmonic part for declination, horizontal intensity, and dip. Two are sufficient to eliminate the harmonic part in vertical intensity. For the same reason the headings need not be restricted to cardinal or intercardinal points. It is necessary, however, and it is sufficient that they be equally spaced

around the compass-points. This is worth considering especially when swinging under sail only. The time taken to make a swing can probably be reduced still more by the adoption of electrical methods for determining inclination and intensity. Experience on the Carnegie confirms both the practicability and the rapidity of electrical methods for determining inclination and the method for horizontal intensity had reached a very promising stage of development.

It appears from these considerations that satisfactory determinations for secular variation can be made on a vessel not especially constructed for magnetic work and that the reduction need not necessarily involve so much labor in observing and computing as was required for such vessels on past expeditions. But trial is necessary to substantiate these conclusions. Some vessels, even of wooden construction, may be too highly magnetic for reliable work. Having found one that is suitable, every precaution should be taken against large changes in her magnetic condition.

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EARTH-INDUCTOR MEASUREMENTS ABOARD THE CARNEGIE, CRUISE VII

Floyd M. Soule

On the recently ended last cruise of the non-magnetic vessel Carnegie the earth inductor was used for the routine measurement of inclination and, experimentally, for the measurement of the horizontal intensity of the Earth's magnetic field.

Inclination

Figure 1 illustrates the set-up used for the measurement of inclination. Here, the earth inductor (EI) mounted on double gimbals, carried a commutator-coil which was driven at constant speed by a tuning-fork-controlled rotary converter located in the after part of the vessel. The drive was transmitted by bronze shafting and through a universal joint at the center of gimbal-motion.

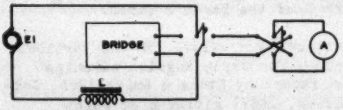


Fig. 1--Earth-inductor set-up for inclination

The earth-inductor coil was fitted with a two-segment commutator upon which rode plain strip brushes of phosphor bronze. A twisted pair of conductors led through a conducting shield from the earth inductor in the forward dome to the control-room on the quarter-deck. Here the circuit passed through a coil (L) of high inductance and low resistance to a double-pole double-throw switch, to one side of which was connected, through a reversing switch, a direct-current microammeter, and to the other side a portable Wheatstone bridge.

With the circuit closed by the microammeter, the average current flowing depended on the position of the coil in the Earth's field and the dimensions of the coil, and was proportional to its speed of rotation and inversely proportional to the total resistance of the circuit. As a check on the constancy of the resistance of the circuit, the resistance of that part which is external to the microammeter was measured by the Wheatstone bridge at the beginning, at the

mid-point, and at the end of the observations. This measurement was made of course with the earth-inductor coil stationery.

The purpose of the inductance, L, was to smooth out the fluctuations in the current. These fluctuations arose from two causes. One was the periodic fluctuation in speed of rotation of the coil when the portions of the drive-shaft on each side of the universal joint were not parallel. The other cause was the oscillation of the gimbal-stand about the true vertical. Because of the difference between the cosine of an angle θ, and the mean of the cosines of $(\theta + \Delta\theta)$ and $(\theta - \Delta\theta)$, the average current passed by the inductance was not accurately independent of the motion of the gimbal-stand, but the inaccuracy was small enough to be neglected in marine measurements.

With the instrument oriented so that its vertical circle faced magnetic east, and with the rotation-axis of the coil free to be moved only in the vertical plane of the magnetic meridian, the rotation-axis of the coil was offset from the position of inclination until a current of three microamperes was indicated. The vertical-circle reading was taken and the coil then moved in the plane of the magnetic meridian until a current of three microamperes in the opposite direction was indicated. The vertical circle was again read and the mean of the readings constituted a measurement of inclination. These operations were then repeated for currents of two microamperes and one microampere. The graduated ring of the gimbal-stand was then turned through 180°, the rotation-axis of the coil was turned through approximately $(180 - I)°$, and a similar set of observations was made. These twelve observations gave six measurements of magnetic inclination, I, from which a mean value was taken. The values of inclination thus obtained were reported to nearest one-tenth degree, although the error was considered to be less than this under the most severe conditions at sea. In quiet water this method used on board has checked with observations on shore to one-half minute.

Horizontal intensity

The measurement of magnetic horizontal intensity by the earth inductor presented greater practical difficulties than did the measurement of inclination. Routine measurements of horizontal intensity were made with the deflector, which was located in the after dome of the Carnegie. The rapidity of both deflector and earth-inductor measurements was to some extent a variable dependent on the motions of the vessel as affected by the weather. However, under weather conditions which required two hours for a complete deflector-determination of horizontal intensity, about half that length of time was necessary for the complete determination of inclination. It will be seen that a short period of time, after completing the routine inclination-measurement, was available for experimental earth-inductor measurements of horizontal intensity simultaneously with the routine deflector-measurements. The intensity-measurements of the deflector were used as a control for the experimental work of the earth inductor.

The first method of measuring horizontal intensity with the earth inductor on Cruise VII was selected for its simplicity. The set-up used was the same as indicated in Figure 1. The rotation-axis of the earth-inductor coil was vertical and the current commutated when the plane of the coil was perpendicular to the magnetic meridian. The commutated current set up in the closed circuit by the electromotive force induced in the rotating coil was directly metered by the direct-current microammeter. Under these conditions the horizontal intensity (H) was given by the formula

$$H = C\left[\frac{R \times 10^8}{\frac{4\pi n}{3}\left(\frac{r_2^3 - r_1^3}{r_2 - r_1}\right)N}\right]$$

where C was the current in microamperes read from the direct-current microammeter, R the total resistance of the circuit in ohms, n the speed of rotation of the coil in revolutions per second, r_2 and r_1 the outer and inner radii respectively of the coil in centimeters, and N the number of turns in the coil. N, r_2, and r_1 were constants of the coil; n was considered sufficiently constant and known because the coil was driven by a constant-speed, tuning-fork-controlled rotary-converter; and R was obtained as the sum of the microammeter-resistance computed from a basic known resistance and its temperature-coefficient, and the resistance of the circuit external to the microammeter as measured by the Wheatstone bridge before and after each current-measurement.

This method gave erratic results, was not particularly rapid, and had some outstanding disadvantages which it was desirable to eliminate. Consequently the current-method was abandoned and another method was used. This gave far more satisfactory results and consisted of the direct measurement of the generated electromotive force by means of an ordinary Leeds and Northrup type K direct-current potentiometer. Here the microammeter was used as a galvanometer for indicating the potentiometer-balance and was depended upon as a current-integrating instrument. The set-up used differed from that indicated in Figure 1 only in that the output from the reversing switch was led to the "EMF" terminals of the potentiometer. The electromotive force at the terminals of the earth inductor consisted of a unidirectional series of half sine-waves. At a condition of balance current would flow but the time-integral of the current in one direction was equal to the time-integral of the current in the opposite direction. This statement assumes perfect current integration by the microammeter. The inductance, L, here acted to reduce the amplitude of the fluctuations of the current about the mean. Assuming that the fundamental frequency of this fluctuation was twice the frequency of rotation of the coil the inductance presented a reactance to the fundamental of about 50,000 ohms whereas the total resistance of the circuit was about 300 ohms.

The procedure in measurement was to set the earth inductor coil-axis vertical with the brushes commutating when the coil was cutting no flux. A set of three readings was taken with the potentiometer. The horizontal circle of the earth inductor was then turned through 180°, the reversing switch was thrown in the opposite direction, and a second set of three readings taken. The gimbal-stand circle was then turned through 180°, the reversing switch again thrown, and a third set of three readings taken. The horizontal circle of the earth inductor was then turned through 180°, the reversing switch thrown, and a final set of three readings taken. The twelve readings were meaned for a determination of horizontal intensity.

The four different positions of the earth inductor were used to balance out irregularities of construction and lack of balance of the earth inductor, while the reversals thus occasioned balanced out such thermo-electromotive forces and contact-potentials outside the potentiometer as were steady, the multiplicity of readings taking care, to some extent, of the unsteady extraneous electromotive forces.

Between our departure from Callao, Peru, February 5, 1929, and our arrival at Pago Pago, American Samoa, November 18, 1929, between 80 and 90

simultaneous comparisons were made between this method and the deflector at sea, under a variety of sea-conditions, on all headings, at temperatures ranging from 12° C to 33° C, at inclinations varying from 0° to 67.5, at horizontal intensities from 0.201 gauss to 0.360 gauss, on magnetically disturbed and quiet days, and at stations having a wide geographical distribution. With but few exceptions the two methods checked to within one-half of one per cent. The ratios of deflector and earth-inductor values were all plotted against both temperature and inclination, but in neither case could a relation be shown. Some of the differences between deflector and earth-inductor values may have arisen from changes in coil-dimensions caused by expansion and contraction of the bakelite spool with changes in humidity. This difficulty, if it existed, might be eliminated by winding a coil on a marble spool.

Unfortunately, the experimental data was lost with the Carnegie, but as the writer was himself occupied with this work he is reasonably sure of the above general statements of the performance. The records of the only direct comparison between this method and magnetometer-measurements ashore are preserved. These measurements were made on July 31, 1929, when the Carnegie was swung in San Francisco Bay in the vicinity of Goat Island. This comparison is shown in Figure 2. Observatory traces from both Huancayo (Peru) and Mt. Wilson show disturbances during the comparison. In addition, there is the possibility of local disturbance indicated by the greater amplitude of the Goat Island measurements. In any event, the agreement between the earth-inductor and magnetometer values is very encouraging and seems to be better than the one-tenth of one per cent which had been considered the best which could be hoped for with the type of speed-control used. These shipboard measurements, however, were made under ideal conditions.

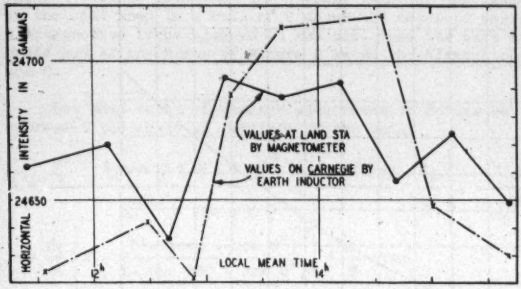


Fig. 2--Results San Francisco Bay, July 31, 1929

The chief need, which existed at the time the cruise was terminated, was for a more sensitive balance-indicator. It should be borne in mind, in designing further improvements, that any new instrument must contend with the effect of the ship's motion on any moving element. Steady motions of translation can of course be neglected. However, those motions involving acceleration, particularly roll and pitch, affect the accuracy of the earth-inductor method of measuring horizontal intensity in two ways, namely, in increasing the difficulty of determining the mean position of the microammeter-needle, and in producing departures from level of the earth inductor on the gimbal-stand. The motion of the coil-axis relative to the true vertical can be resolved into two components--one in the vertical plane including the magnetic meridian and the other in the vertical plane perpendicular to the first.

In the first case, the effect is partially compensating, and the error introduced arises from the previously mentioned difference between a trigono-

metric function of an angle θ and the mean of the same functions of $(\theta + \Delta\theta)$ and $(\theta - \Delta\theta)$. In the measurement of inclination these errors are negligible because during any one set of observations the same errors will occur in each direction and will, to a large extent, cancel each other.

In the measurement of horizontal intensity, however, an absolute, rather than a relative, value is determined. In this case we will have an error introduced by the swing of the gimbal-stand in the vertical plane of the magnetic meridian. This error at any instant will be, proportionally

$$\{\cos I - [\cos (I + \Delta I) + \cos (I - \Delta I)]/2\}/\cos I$$

where I is the angle of inclination and ΔI is the angular displacement of the gimbal-stand from its normal position. The magnitude of the proportional error is independent of I and will depend only on ΔI. The motion of the gimbal-stand is oscillatory and if it be assumed simple harmonic then the instantaneous error produced by a displacement of ΔI will be the average error produced by a swing whose maximum displacement is $\Delta I(\pi/2)$

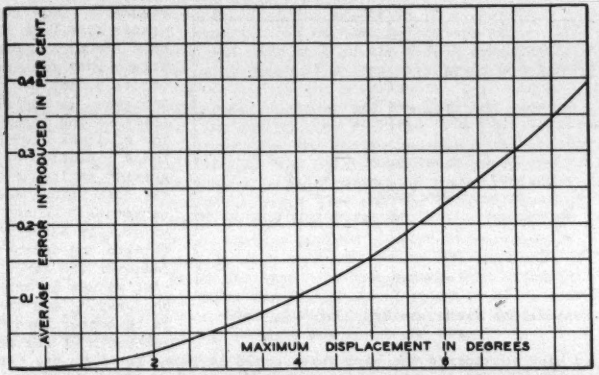


Fig. 3--Error due swing gimbal-stand in plane magnetic meridian

The manner in which the average error varies with the maximum displacement of the coil axis from the true vertical is shown in Figure 3. From this it will be seen that a maximum displacement of about 4° is necessary to introduce an error of one-tenth of one per cent. This indicates that the ordinary motions of the gimbal-stand in this plane need not greatly concern us in marine measurements.

There remains to be considered the effect of motion in the vertical plane which is perpendicular to the vertical plane of the magnetic meridian. Referring to Figure 4, let I = inclination, F = total intensity = OA, Z = vertical intensity = CA, H = true horizontal intensity = OC, and H_1 = the projection on the normally horizontal plane of the earth inductor when rotated about the magnetic meridian through an angular displacement of θ. The angle between H and H_1 is ϕ from which $(H_1/H) = \sec \phi$. H_1 would be the quantity measured by the earth inductor if the brushes were shifted about the coil-axis AB by an angle ϕ so that H_1 would be perpendicular to the plane of the coil at the time of commuta-

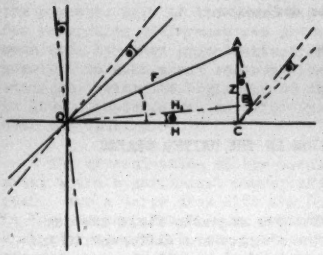
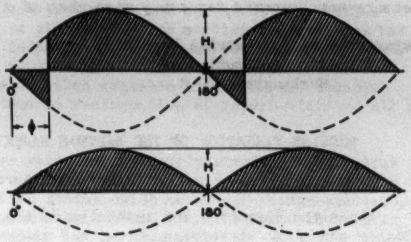


Fig. 4



Figs. 5 and 6

tion. However, the brushes remain in their original vertical plane so that commutation takes place when the plane of the coil is perpendicular to H. Thus it will be seen that the average value of the electromotive force induced in the coil when in a roll of θ about the magnetic meridian is to the average electromotive force induced in the coil when its axis is vertical as the algebraic sum of the areas of Figure 5 is to the algebraic sum of the areas of Figure 6.

Let this ratio of average electromotive forces be called A. Then if ψ represent the angle of rotation of the coil

$$A = \frac{\int_0^{180^\circ} H_1 \sin\psi \, d\psi - 2\int_0^{\phi} H_1 \sin\psi \, d\psi}{\int_0^{180^\circ} H \sin\psi \, d\psi} = \frac{H_1}{H}\left[1 - \frac{2\int_0^{\phi}\sin\psi \, d\psi}{\int_0^{180^\circ}\sin\psi \, d\psi}\right]$$

$$= \frac{H_1}{H}\left[1 - \frac{2(-\cos\phi + \cos 0^\circ)}{(-\cos 180^\circ + \cos 0^\circ)}\right] = \frac{H_1}{H}\cos\phi$$

But since $(H_1/H) = \sec\phi$, we have A = 1, which means that the average value of the electromotive force induced in the coil is independent of the roll in the vertical plane perpendicular to the magnetic meridian. The only error introduced, then, by such a motion will be caused by the failure of the potentiometer and microammeter to accurately measure the average electromotive force induced in the coil.

In conclusion, it may be said that the potentiometer-method was superior to the current-method because it relied on standards of resistance and electromotive force embodied in the potentiometer and standard cell instead of standards of resistance and current embodied in the portable bridge and microammeter. Because of differences both in theory and construction, the potentiometer is less likely to change than is the portable bridge, and the standard cell is far more permanent than the microammeter-calibration. In the current-method there was no assurance that the brush-resistance was the same with the earth inductor running as it was when stationary. The potentiometer-method had the further decided advantage that changes in resistance of the circuit affected, not the accuracy, but only the sensitivity of the measurement.

It is recognised that the field for improvement is great, although it is

felt that toward the end of the cruise the accuracy of the earth-inductor measurement compared favorably with that of the deflector.

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SECULAR VARIATION OF THE EARTH'S MAGNETISM IN THE UNITED STATES

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Since the direction and intensity of the Earth's magnetic field are constantly changing, at varying rates, and since the changes are different in different places, it follows that as an essential part of a magnetic survey observations must be repeated at selected stations suitably distributed over the country in order to determine the changes with lapse of time and thus make it possible to refer to a common epoch the results of observations made at different times. This is particularly important in a large country like the United States where the magnetic survey involved the occupation of more than 5,000 stations and was necessarily spread over a period of years. Magnetic observatories must be maintained for a continuous record of the variations and for control of the field observations of the survey, but these are necessarily very limited in number.

Aside from the question of funds available, various factors must be taken into account in deciding upon the number of repeat-stations and the interval between successive reoccupations. There are indications of an eleven-year period in the secular variation, corresponding to the sunspot-cycle, and that period has been recommended as the proper interval for repeat observations. It is found, however, that more pronounced features of the secular motion sometimes develop in a shorter interval than eleven years. On the other hand, if the time-interval is as short as two or three years the errors of observation may unduly increase the uncertainty of the derived rates of change for the interval. Five years has been selected by the Coast and Geodetic Survey as the most desirable interval.

While the secular variation is no doubt affected to some extent by local conditions, and this effect may become considerable in the case of marked magnetic anomalies, it is found that in general the secular variation changes slowly and progressively in going from place to place. At two places 50 miles apart, the secular variation will ordinarily agree in essential features, so that the results at a repeat-station may be used for finding the change within a considerable area surrounding it, and the change at any place between two repeat-stations may be found by interpolation. For this reason it is possible to have the repeat-stations rather widely distributed, provided exact reoccupation and undisturbed conditions are assured. Unfortunately, however, the growth of towns or the introduction of electric railways or other sources of artificial local disturbance frequently detract from the value of the secular-variation data at a station. For this reason it is necessary to provide for a somewhat closer distribution in order to guard against an occasional loss of station. Stations 150 miles apart will generally suffice for all purposes and a much wider distribution will meet many needs. For the United States this involves the reoccupation of about 200 repeat-stations at intervals of about five years. Under certain conditions interpolation can safely be extended over wider intervals, but the conditions are different for the different elements and different also for different times.

The importance of repeat-observations was recognized at an early date in the magnetic work of the Coast and Geodetic Survey and in selecting stations for occupation preference was given to places at which earlier observations had been made by other organizations or individuals, though it was not, in general, possible to make exact reoccupation in the absence of descriptions of the old stations. With the beginning of the detailed magnetic survey of the country in 1899, provision was made for systematic reoccupation of repeat-stations all over the country.

The determination of the secular variation is fairly simple for a limited area, where a sufficient number of repeat-stations can be occupied in a single year. For a large area like the United States, with about 200 repeat-stations, it has not been feasible to make repeat-observations over the whole country in a single year, nor to occupy the stations in the same order at successive reoccupations. On the basis of repeat-observations once in five years, approximately one-fifth of the stations are occupied each year and the time-interval may be anywhere from three to seven years. This introduces an element of heterogeneity in the data which add materially to the difficulty of finding for the whole country the change for the specified 5-year period.

In view of the necessarily long period occupied in the execution of the magnetic survey of the United States, publication of the accumulated results has been made from time to time before its completion, in the form of magnetic tables and magnetic charts. For each publication it was necessary to prepare suitable secular-variation tables in order to reduce the observed results to the epoch for which the magnetic charts were to be constructed.

An examination of the collected results of declination for stations at which observations cover a long period (100 years or more) indicates that the change in time is periodic in character, at least it does not go on indefinitely in one direction, and in the earlier discussions by Schott and others of such heterogeneous results as were then available it was found possible to represent fairly well the observed changes at a station by an analytical expression with a single periodic term, the period and amplitude differing from station to station. It was recognized, however, that a formula derived in this way is purely empirical and can not be used for extrapolation.

With the accumulation of more accurate and homogeneous data it became evident that the observed results could not be represented with sufficient accuracy by any simple formula. The discrepancies became especially noticeable subsequent to 1902, at which time there was a marked change in the character of the secular variation of declination. In the later discussions, therefore, some graphical method has been used to derive secular-change tables representing the later observations, retaining the empirical formulas, however, to extend the tables prior to 1840.

In the preparation of the United States Magnetic Tables and Magnetic Charts for 1915 the following method was adopted: The results at all the repeat-stations in a limited area, usually a whole state, were combined to form a single series of values representing approximately the average conditions for the whole area. A curve was then drawn with time as abscissa and magnetic element as ordinate, showing graphically the values in this series. Before the final adoption of such a curve of secular variation for a particular region, comparison was made with the curves for adjacent regions as a basis for the smoothing out of irregularities. From these curves values were scaled off at 5-year intervals, forming tables showing the change in that magnetic element from each date to 1915. Each of these tables applied roughly to the center of

the region from which it was derived. From them other similar tables, as needed, were computed by interpolation according to latitude and longitude.

In the case of United States Magnetic Tables and Magnetic Charts for 1925, published in 1929, a different method was used. For the 10-year interval 1915-1925 values of annual change were computed for every station at which suitable repeat-observations had been made. For only a few stations were results available for just the selected 10-year interval, but use was made of every reasonable approximation to it. Thus, observations in 1914 and 1926 or 1916 and 1924 or 1917 and 1923 would give a value of annual change application to the interval 1915-1925, and 1914 and 1924 or 1916 and 1922 would serve almost as well. For intervals such as 1914 to 1921 or 1918 to 1926 it was necessary in some cases to take into account the variation in the rate of change during the 10-year interval, as found by a subsequent discussion of the two halves of the interval separately.

These values of annual change were then plotted on a base-map of the United States and lines of equal annual change were drawn representing the plotted values approximately, but without attempting to follow all the indicated irregularities. The amount of change for the 10-year interval could then be found for any place in the country by interpolation between these lines. Similar maps were prepared for other 10-year intervals 1905-1915, 1895-1905, etc., and then in some cases the 10-year interval was divided into two 5-year intervals when it appeared that the rate of change was materially different in the two halves.

With the aid of these maps it became possible to prepare a table for any place in the United States, showing the change from successive dates at intervals of five or ten years to 1925. For the purpose of reducing the observed values to January 1, 1925, such tables were prepared for places at intervals of 2° in latitude and 5° in longitude in the case of dip and horizontal intensity. For declination the tables were spaced 2° apart in longitude as well as in latitude.

For the purpose of reducing the observed values of declination, dip, and horizontal intensity to 1925 it would have sufficed to extend these tables back to 1875, but it has seemed desirable to go as far back as the earliest observations in order to present in this more convenient form all the available information regarding the secular change. In the case of declination the data had been discussed so thoroughly before that further study of the earlier observations was unnecessary. For dip and horizontal intensity, however, it was decided to examine again the earlier observations and apply to them the above method of determining the change for successive 10-year intervals. Along the eastern fringe of the country these observations go back to the 1840-1850 decade.

These maps of lines of equal annual change are given on the accompanying diagrams for the four 10-year periods 1885-1895, 1895-1905, 1905-1915, 1915-1925. It is an outstanding feature of all of them that the rate of change is quite different in different parts of the country and different in different decades. In the case of declination it will be seen that for the first two of these decades the general trend of these lines was north and south, in the third it was northwest and southeast, while in the fourth it was more nearly east and west. The line of no change moved to the eastward for three decades, more rapidly in the southern part of the country than in the northern, and then it swung around to an east-west position in the southern part of the country. In New England the rate of change increased to about 6' in the 1905-1915

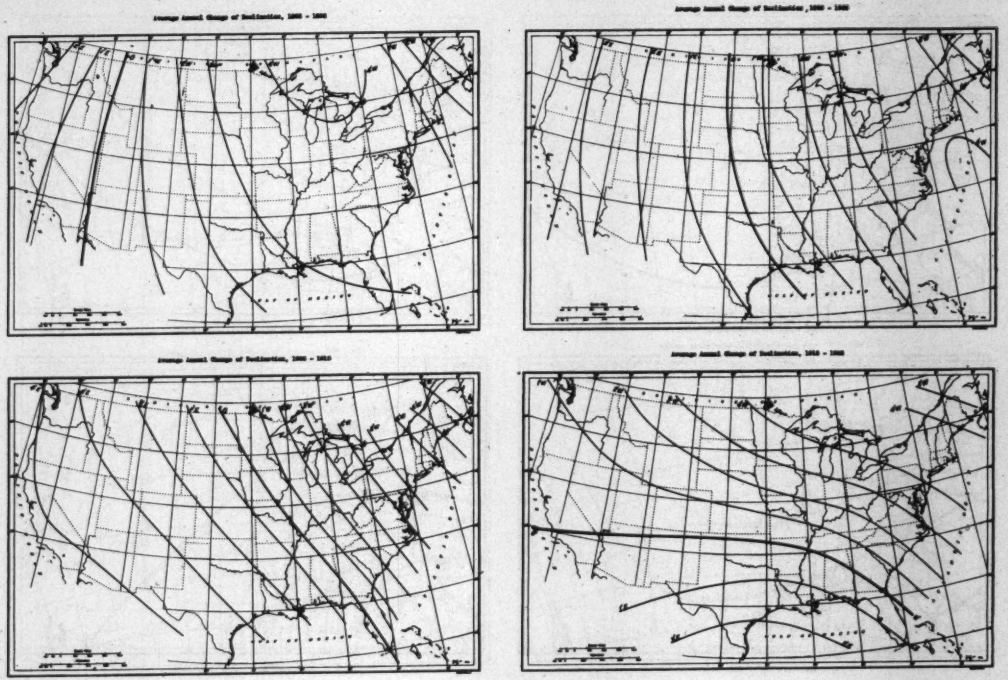


Fig. 1

FIG. 2

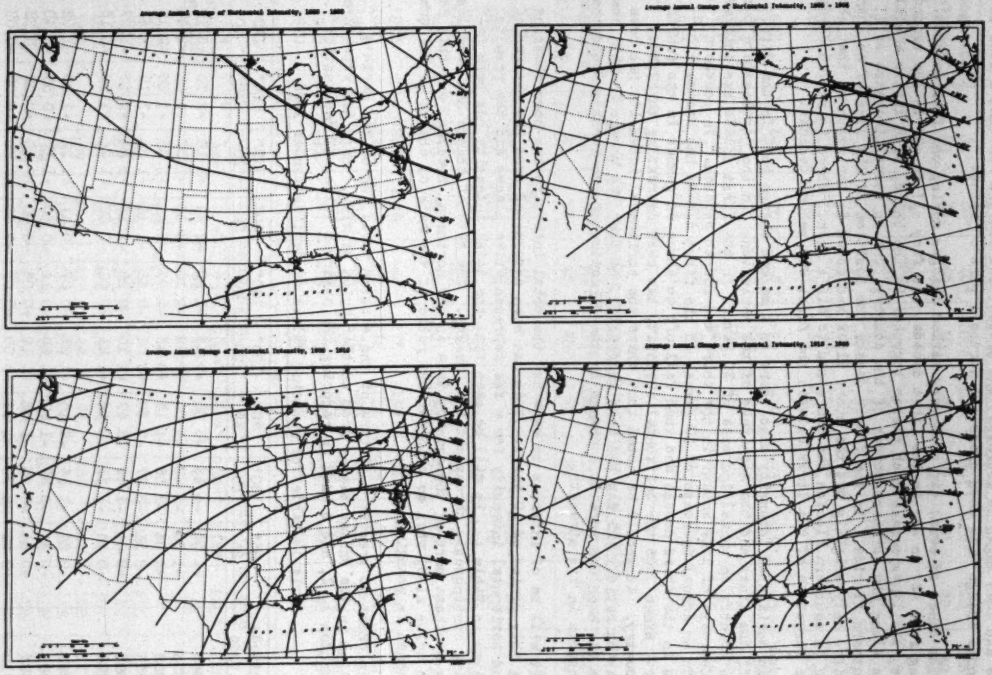


Fig. 8

decade and then began to decrease, and present indications are that a line of no annual change will soon cross the northern part of Maine.

The lines of equal dip show very marked changes during the four decades. Ranging from -2' to +1' in the first decade, and from -1' to +3' in the second decade, the lines became much closer together in the third, with little annual change along the northern border of the country, but an annual increase of 7' in southern Florida. In the fourth decade the position of the line of no change had moved only slightly but the other lines had moved to the south so that the change in southern Florida had decreased to 5'.5 a year. There is marked change in the curvature of the lines between the first and second decades.

In horizontal intensity also there is a marked change both in the direction of the lines and the magnitude of the variation. In the northeastern part of the country an annual increase of 20 gammas in the first decade has changed to a decrease of like amount in the third and fourth decades. In southern Florida an annual decrease of 30 gammas in the first decade had become 110 gammas in the third decade and then fell off to about 95 gammas in the fourth decade. Since 1905 the horizontal intensity has been decreasing throughout the country. It should be noted that where the horizontal intensity has been decreasing rapidly the dip has been increasing, so that the change in this component has been due more to a change in the direction of the Earth's field than to a change in its strength.

It will be of interest to know how the total intensity has been changing during this period of 40 years. This has been determined from the changes of dip and horizontal intensity for a few representative places and the results are given in Table 4. It will be seen that the total intensity has been decreasing continuously throughout the country. The rate of change has been less in the southern part of the country than in the northern, however, and this relation holds also, when we consider the ratio of the rate of change to the quantity which is changing, $\frac{dF}{F}$.

In order to present a picture of the secular variation of the Earth's magnetism in the United States from another point of view, there are reproduced here Tables 1, 2, and 3 used in reducing the observed values of declination

Table 1--Table for reducing values of declination to 1925

Longitude Latitude	85° 25	80° 35	75° 45	100° 25	100° 40	100° 50	115° 30	120° 40	125° 50
	° '	° '	° '	° '	° '	° '	° '	° '	° '
1845	-1 56	-4 13	-5 35	+0 46	-1 10	-2 24	+2 18	+2 30	+3 19
1855	-1 33	-3 42	-4 55	+0 44	-1 10	-2 22	+1 57	+2 02	+2 44
1865	-1 04	-3 06	-4 15	+0 45	-1 03	-2 11	+1 36	+1 35	+2 10
1875	-0 31	-2 27	-3 30	+0 52	-0 46	-1 48	+1 24	+1 19	+1 47
1885	+0 01	-1 49	-2 49	+1 10	-0 18	-1 13	+1 24	+1 13	+1 27
1890	+0 16	-1 31	-2 34	+1 21	-0 01	-0 52	+1 26	+1 12	+1 16
1895	+0 27	-1 14	-2 16	+1 23	+0 07	-0 41	+1 22	+1 03	+1 02
1900	+0 39	-0 57	-1 59	+1 25	+0 14	-0 30	+1 17	+0 55	+0 48
1905	+0 34	-0 43	-1 42	+1 10	+0 06	-0 40	+0 53	+0 32	+0 25
1910	+0 29	-0 30	-1 14	+0 50	-0 05	-0 48	+0 28	+0 08	-0 03
1915	+0 19	-0 19	-0 45	+0 27	-0 12	-0 39	+0 06	-0 05	-0 14
1920	+0 10	-0 09	-0 25	+0 12	-0 11	-0 27	-0 01	-0 06	-0 11

Table 2--Table for reducing values of dip to 1925

Longitude / Latitude	85° 25	80° 35	75° 45	100° 25	100° 37	100° 49	115° 31	120° 37	125° 49
	'	'	'	'	'	'	'	'	'
1845	+156	+37	-65						
1855	+150	+34	-69	+136	+50		+48	+14	
1865	+150	+38	-62	+118	+41		+39	+12	
1875	+147	+44	-49	+101	+34	-16	+33	+9	-23
1885	+156	+61	-29	+102	+40	-9	+29	+5	-26
1890	+153	+66	-17	+ 98	+40	-6	+25	+1	-27
1895	+147	+70	-7	+90	+39	-3	+19	-3	-27
1900	+137	+68	-1	+80	+34	-2	+17	-1	-23
1905	+119	+60	-1	+64	+25	-1	+15	+2	-15
1910	+86	+45	-1	+44	+16	0	+11	+2	-8
1915	+50	+28	-2	+25	+9	+1	+6	+3	-3
1920	+24	+13	-1	+11	+4	+1	+3	+1	-1

Table 3--Table for reducing values of horizontal intensity to 1925

Longitude / Latitude	85° 25	80° 35	75° 45	100° 25	100° 37	100° 49	115° 31	120° 37	125° 49
1845		-200	+10						
1855	-380	-200	+10				-205	-120	
1865	-355	-203	-8				-175	-110	
1875	-322	-210	-16	-238	-135	-36	-145	-100	-38
1885	-288	-192	-32	-207	-131	-36	-120	- 81	-28
1890	-270	-183	-37	-191	-124	-35	-108	- 72	-23
1895	-251	-182	-45	-175	-119	-35	-99	-65	-19
1900	-228	-172	-54	-158	-111	-36	-91	-61	-21
1905	-196	-153	-54	-136	-96	-36	-80	-55	-23
1910	-156	-120	-43	-105	-73	-28	-61	-44	-21
1915	-95	-71	-26	-67	-45	-18	-40	-30	-16
1920	-43	-30	-10	-30	-20	-9	-19	-14	-8

Table 4--Table for reducing values of total intensity to 1925

Longitude / Latitude	85° 25	80° 35	75° 45	100° 25	100° 37	100° 49	115° 31	120° 37	125° 49
1845		-369	-418						
1855	-340	-381	-448				-262	-214	
1865	-297	-372	-471				-229	-199	
1875	-247	-366	-406	-198	-204	-286	-188	-187	-232
1885	-169	-252	-326	-146	-171	-229	-152	-158	-217
1895	-125	-192	-224	-117	-146	-177	-137	-148	-194
1905	-89	-158	-219	-103	-143	-165	-112	-112	-155
1915	-60	-74	-115	-64	-78	-71	-59	-56	-64

dip, and horizontal intensity for nine representative points, three along the Atlantic Coast, three along the 100th meridian, and three along the Pacific Coast. For comparison there has been added Table 4 showing the change of total intensity at the same places, obtained as indicated above. These tables serve to bring out again the marked differences in character of the secular variation in different parts of the country and at different times.

In the declination-tables a plus sign indicates that the north end of the compass-needle pointed further to the east in 1925 than it did at the earlier date, the value on any line being the total change from that date to 1925. In the northeastern part of the country west declination has been increasing steadily since 1845, the change in the 80 years being 5° 35' at the selected point. For the extreme northwestern part of the country, on the other hand, east declination was increasing steadily up to about 1915, the total change being 3° 33'.

Along the North Atlantic coast there are available several values of declination, necessarily rough approximations, dating back to the beginning of the seventeenth century and Schott was able to collect enough values around 1700 and 1750 to get a general idea of the distribution of declination along the Atlantic coast for those epochs as a basis for carrying the discussion of the secular variation back to that time, supplemented by other early results from various sources. Boston may be taken as an illustration of the results of his and subsequent discussions. There the declination decreased from over 11° west in 1670 to less than 7° west in 1785 and since then it has been increasing steadily until it is now about 15° west. The date at which this change from easterly to westerly motion occurred comes progressively later as one goes to the west--about 1770 in Maine, 1800 in New York, 1830 in Indiana, and 1850 in Nebraska.

Prior to 1885 the change in dip was small for the greater part of the country, with some decrease in the northeastern part of the country and an increase in the southwestern portion. Since 1885 it changed but little in the northern and western parts of the country, but toward the southeastern part the rate of increase gets steadily larger.

In the intensity-tables the quantities are expressed in units of the fourth decimal place, C.G.S. system. The predominant feature has been a decrease in both horizontal and total intensity, though there has been comparatively little change of the former in the northern belt of the country. Between 1675 and 1925 the total intensity decreased about five per cent on the average over the whole country.

From what has been said, it will be seen that prediction more than a few years in advance of observation is futile and that it is not even possible to infer from observations at one station what has been going on at another station remote from the first. It is only by systematic and widespread repeat-observations that an adequate knowledge of the secular variation can be maintained.

U. S. Coast and Geodetic Survey,
Washington, D. C.

REGIONAL DISTRIBUTION OF MAGNETIC SECULAR-VARIATION OVER THE EARTH'S SURFACE

H. W. Fisk

The term "secular variation" is generally used to signify the slow change in the Earth's magnetic field which requires centuries for its full development. In but few places are there records from which a reliable knowledge of these changes may be gained for any considerable period of the past, and such records are confined very generally to one element only, the declination. But aside from changes in the Earth's field considered as a whole there are regional peculiarities which undergo changes of a much more rapid nature, permitting a more complete investigation from records of comparatively few years, and promising to yield vital information not only to the subject of terrestrial magnetism but to other allied branches of geophysics, dealing with forces at work in the interior of the Earth.

The accepted time-unit by which secular variation is expressed is the year, although when the phenomenon is considered in its most comprehensive sence, a larger unit should probably be used in order to minimize the effect on the rate produced by minor irregularities in the year-to-year differences, and the fluctuations which run a course of several years, and may or may not be periodic. The widespread interest in terrestrial magnetism which was aroused about the beginning of the present century resulted in the establishment of numerous observatories, and the inauguration of extensive magnetic surveys, so that except for the polar regions more or less complete data for deriving the amount of change in the Earth's field over a period of years are available.

If the summary of annual means of the magnetic elements supplied by any observatory be taken, the differences for successive years will represent the best obtainable value of the annual change at that station. An inspection of these successive differences shows that they are far from regular, and only by grouping or smoothing by some process can the general trend of the change be obtained. To illustrate the nature of these irregularities and fluctuations, the changes in declination at three observatories are shown graphically in Figure 1.

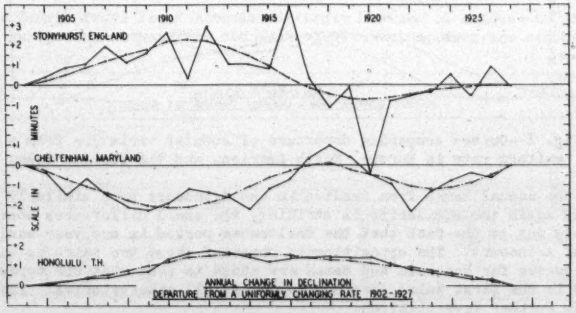


Fig. 1--Departure of successive differences of annual mean declination-values from a uniform rate

Thus at the Stonyhurst, Cheltenham, and Honolulu observatories the smoothed annual rates for 1903.0 and 1927.0 are +2'.9 and +13'.2, -3'.0 and -3'.0, and +0'.8 and +1'.0, respectively. The resulting mean accelerations are +0'.43, 0'.00, and +0'.008 for the three observatories. If it is assumed that the annual rate changed uniformly from that of the first date, 1903.0 to that of the second date, 1927.0, and a rate be computed for the intermediate years on that assumption, the irregularity will be shown by comparing this assumed rate with the successive differences taken from the table of mean annual values. The departure of each of the successive differences from the uniformly changing rate is plotted in the figure, and through several points the smoothed curves are drawn. It is obvious that in many cases the rate of change obtained by comparing two values separated by a period of years, is not the proper one to be applied to the middle of the period, on account of the irregularities and of the variable accelerations which alter it.

That the accelerations may affect large regions in a similar way, but are regional rather than universal in character is indicated by a preliminary study which has been begun of the declinations at a few of the European observatories and of some others for comparison. The departures of the annual mean values from the uniformly changing rate were smoothed by least-square equations involving sine functions, and the resulting curves for the European observatories, while differing in some details, were nevertheless strikingly similar. Two such curves from the widely separated observatories at Coimbra and Pavlovsk are shown in Figure 2 and are seen to be almost identical. The curves similarly

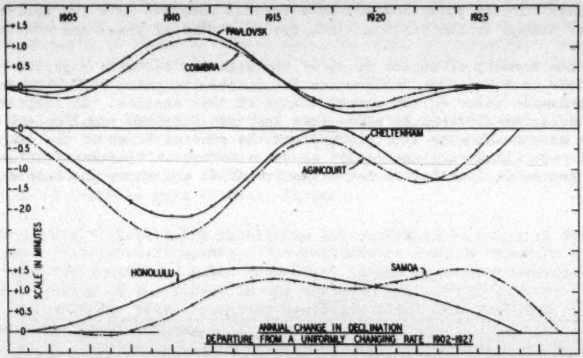


Fig. 2--Curves comparing departure of secular variation from a uniform rate in Europe, North America, and the Pacific Ocean

found from the annual means from Cheltenham and Agincourt were similarly constructed and again the similarity is striking, the small differences observed being largely due to the fact that the Cheltenham period is one year earlier than that of Agincourt. The opposition in phase of these two pairs is suggestive. The curves for Honolulu and Samoa are added to show that the apparent periodicity in the first pairs does not appear at all observatories. The subject demands further investigation.

The foregoing discussion of conditions found to obtain at observatories shows that a large degree of uncertainty must be expected when a rate of change

of a magnetic element is obtained from field observations. If the period separating the two observations is short, the irregularities have a serious effect, and if a longer period is chosen the variable accelerations are similarly troublesome. However, if the material is sufficiently abundant, approximations may be made which will reveal the approximate distribution of the secular variation, although all charts representing the simultaneous rates of change over the whole Earth at any particular epoch prepared with the present facilities for collecting data, must be regarded as imperfect.

For convenience, the lines drawn through points of equal annual change are called "isopors" or "isoporic lines." Among the latest isoporic charts of the three elements are those in the "Handbuch der Experimentalphysik" in a chapter by Bartels. On these the tendency of the isopors to form closed ovals around centers or "foci" is clearly indicated. From more abundant data now available it has been possible to prepare charts showing this tendency in greater detail--although, it should be repeated, the positions of the lines in the nature of the case must be regarded as approximate. Much of the material used is susceptible of refinement, such as least-square adjustment, and correction for accelerations, approximated from observatory-records, but it is not believed that such refinement would materially alter the general arrangement. The base upon which these isopors have been drawn is the homolosine equal-area projection of J. Paul Goode, copyrighted by the University of Chicago, and used with permission. The advantages of the equal-area feature with the possibility of including the polar areas more than offset the disadvantages of interruptions between the lobes.

The isoporic declination-chart shows four foci at which the maximum rate of change is greater than ten minutes a year. Attention is called to the great area of westerly change in South America, centering in Brazil, with the weak center of easterly change just west of Central America. Two strong centers, one of easterly change in southeast Africa and the other of westerly change east of Madagascar in the Indian Ocean result in a remarkable crowding of the lines between Mauritius and the continent. Here is evidence of a force of continually increasing intensity being built up in that region which has the effect of drawing the north ends of two needles, if one were placed in the Indian Ocean and the other on the east African coast, toward each other at the rate of one-half degree each year.

The fourth focus is of especial interest because it chances to lie in western Europe where there are abundant observatories, from the records of

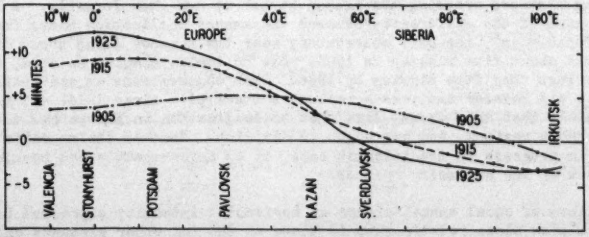
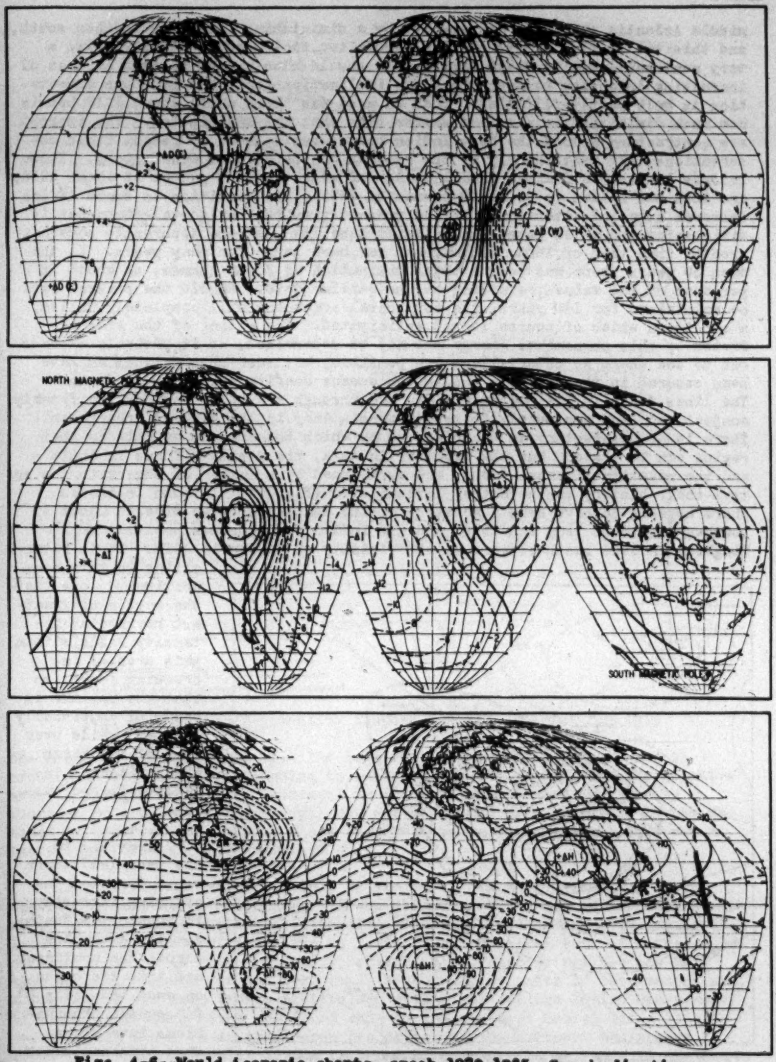


Fig. 3--Variation of the annual change in declination zone 50° to 60° north latitude from Ireland to Siberia, in 1905, 1915, and 1925.

which it can be adequately studied. Inspection of these records shows that no such focus existed at the time most of the observatories were established, between 1900 and 1905, but has appeared since that time. In those years the annual rate of change of declination over most of western Europe was between three and five minutes, while at the present time it has risen to the unusual rate of about 13 minutes at some of the observatory-stations. That this increase has not been uniform has already been shown in Figure 1 in the case of Stonyhurst (England). It is interesting to note that while the rate of easterly movement was rapidly becoming greater in Great Britain and western Europe, it was almost stationary in central Russia at Kazan (or Sloutsk), and in eastern Russia and Siberia the movement was in the opposite direction. This is graphically represented in Figure 3. A line is drawn from western Ireland to central Siberia, which passes near some of the European observatories (longitude indicated by the position of names on the diagram). The annual change in declination along this line as it has been determined for 1905 is plotted and shows an approximate uniformity or very moderate gradient for the entire distance. Ten years later the rate had greatly increased in the vicinity of the North Sea, had remained essentially stationary at Kazan in central Russia, but had diminished at Katherinenburg and Irkutsk. In the succeeding decade the process continued, although by the end of that time and at the present there had been a marked slowing in the rate of acceleration. Thus it appears that the North Sea focus developed since 1905, has at the present reached a maximum, and whether it will remain or disappear is for the future to reveal.

The inclination-isopors like those of declination (Fig. 4) show a tendency to crowd together around foci (Fig. 5). Over the most of the Earth's surface the annual change in inclination does not exceed three or four minutes, but approaching these foci the rate rapidly increases, and at the centers of two, that near Ecuador and that near St. Helena, it exceeds one-quarter degree, while at the Persian center it reaches a possible maximum of about half that magnitude. The remarkably steep gradient along a line across South America from eastern Brazil to western Ecuador deserves especial attention. Unfortunately there are no observatories in this region and the details of the activity here are based on field observations alone. From these it is inferred that the rates are becoming smaller in northern South American countries, and are possibly increasing about the St. Helena focus. Observations along the west coast of Africa in 1913, 1919, and 1926 give a larger annual rate for the later than for the earlier period. The same inference may be drawn from the fact that the zero-isopor, or the line separating these two systems, seems to be drifting westward crossing the Amazon higher up now than formerly. A smoothed curve of the differences between the annual inclination means from Vieques (Porto Rico), the only observatory near the Ecuador focus shows that the rate was about five minutes in 1905, rose to twelve minutes in 1914, and receded to less than five minutes by 1924. Such observations as are available in Colombia and Ecuador indicate a recession there also since 1914. It is thus quite probable that this focus, like that of declination in Europe has developed, reached a maximum, and has begun to diminish. Whether the opposite movements are in progress in the Atlantic area, it is unfortunately not possible to say for lack of the necessary records.

The lines of equal annual change of horizontal intensity expressed in force units show close relationship to those of the two other elements expressed in angular units (Fig. 6). It would be natural to expect that a disturbing force directed downward along the northwestern coast of South America would be accompanied by a manifestation of decreasing horizontal intensity in the Caribbean, and this we find to be the case. Likewise a force directed upward in the



Figs. 4-6--World isoporic charts, epoch 1920-1925, for declination, inclination, and horizontal intensity

middle Atlantic should be accompanied by a diminishing intensity farther south, and this too we find exists centered about two foci, one in Patagonia and a very remarkable one in South Africa. We should also expect to find an area of increasing intensity between these two inclination-foci, but here our expectation is only partly fulfilled. Such an area was there until about 1917 but is now much diminished in size and weaker than what was anticipated. The isopors are contracting around the West Indian focus, as is shown by recent field observations, but particularly by the records of the Vieques Observatory. Those records indicate a maximum annual change of more than 120 gammas at about 1915, and this had fallen to 60 or 70 gammas by 1923. The condition in South Africa deserves especial attention. At the present time the horizontal component is diminishing about 110 gammas annually at Cape Town, a rate apparently somewhat greater than that of 1908, although it has been large for many years. In the past 30 years there has been a total diminution of 3,000 gammas, or about 16 per cent of the value prevailing at the earlier date. Should the present rate be maintained for 140 years, the horizontal component will completely vanish, a condition which of course is not anticipated. The extent of the area affected by this phenomenal change can not be determined, as it probably extends out to sea where no observations are possible; but such observations as have been secured in the Atlantic and Indian oceans confirm the lines as drawn. The lines drawn in the Southern Ocean and through the polar regions are frankly conjectural, and are merely to insure consistency in the general arrangement. There is no observatory in South Africa by which the changes of rate in that region can be traced, but the two foci in Asia, the positive focus in India and the negative Siberian focus, are surrounded by a number of observatories so that their history can be better investigated (Fig. 7). In both instances there appears to have been a movement of the center, that in Siberia about 10° southward between 1906 and 1918, and the Indian focus from a position near Hongkong in 1906 southwestward to the vicinity of Ceylon in 1918. It is interesting in this connection to note that the region of greatest horizontal intensity falls within this area of increasing force, so that the maximum is becoming continually greater, while over most of the Earth the horizontal intensity is decreasing.

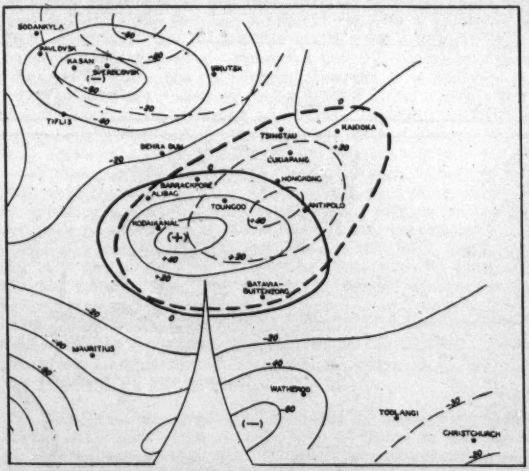


Fig. 7--Change of position of horizontal-intensity isopors in Asia, 1906 to 1918

Previous to the year 1917 there was a focus of increasing intensity in equatorial South America extending across the continent and into the ocean on each side (Fig.8). Subsequent observations have shown that this has disappeared except for the small area near

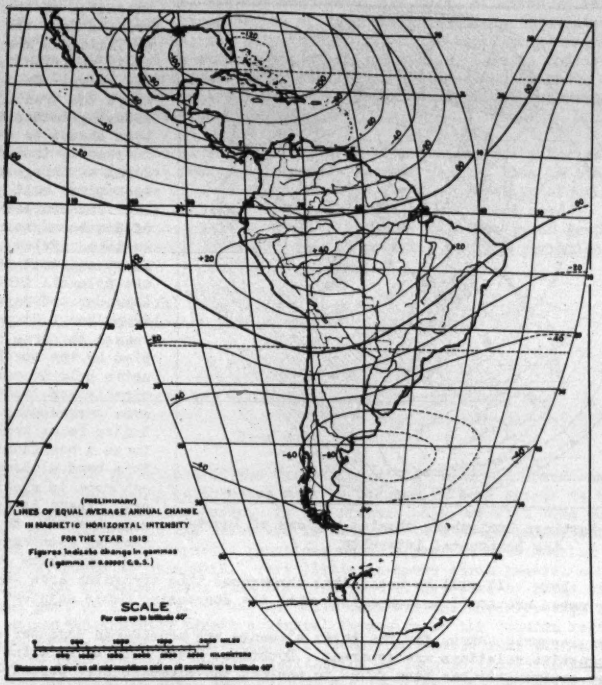


Fig. 8--Horizontal-intensity isopors in Latin America for 1915

the mouth of the Amazon River. The negative area in the Caribbean has increased in area while diminishing in intensity at the center, and the negative area in Patagonia has also increased in size, but whether accompanied by an increase or a decrease in strength can not be stated with certainty in the absence of observatories. Such field observations as are available indicate an increasing force at most stations.

In general, the magnetic field of the Earth has been decreasing during the recent past as is well known. A comparison of the areas of increasing with those of decreasing horizontal intensity, furnishes additional evidence of that fact. Only at the positive focus in India does the annual rate of increase become as great as 40 gammas annually; elsewhere it scarcely reaches one-half that amount. Except for the very small area near the mouth of the Amazon, and a portion of the East Indian Archipelago with a part of the Indian Ocean west of Java, increasing rates are found only in the northern hemisphere. This is conveniently shown on an equal-area projection of the northern hemisphere (Fig. 9). While naturally little is known of secular-change rates in the arctic, the lines have been carried across that area in what seems to be a natural

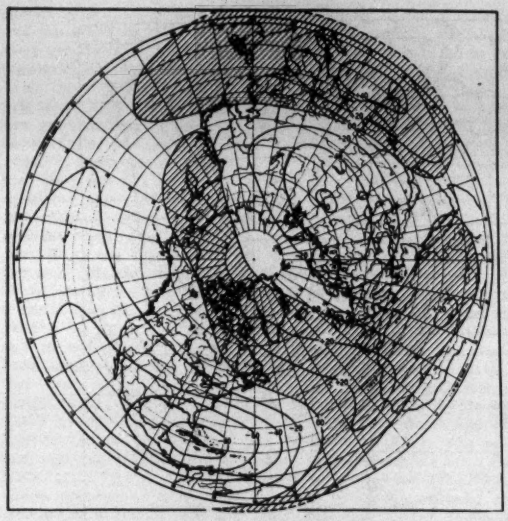


Fig. 9--Northern hemisphere showing regions of increasing horizontal intensity

extension of their courses from the lower latitudes where their position is better known. On the chart the area of increasing intensity has been shaded so as to be readily traced. It forms an irregular and incomplete belt extending from the east coast of South America to northern Africa, thence north and west across the Atlantic including Labrador and part of Greenland. It passes across the area occupied by the north magnetic pole to eastern Siberia and Japan. The area surrounding the Indian focus probably forms a continuation of this band although the evidence is contradictory. There is good reason, however, to believe that an interruption in the continuity occurs in Arabia as shown. It will be noted that throughout this irregular area the increasing rates are small in comparison with the decreasing rates adjacent to it.

When isoporic charts for the three elements are considered together some very suggestive relations are apparent. Doubtless these will be more instructive when opportunity has been found to convert the elements here portrayed into the rectangular components of the total force. The unsymmetrical distribution of the centers of rapid change and their arrangement with reference to the distribution of land and water is noteworthy, the areas of greatest activity lying almost entirely within the hemisphere bounded by the meridians of 90° east and 90° west longitude. The absence of such centers in the Pacific is unquestionably not on account of the difficulty of securing observations over that great area, for enough have been obtained to detect any large change in any element, although they are too meager to discriminate with certainty between the positions of lines of feeble change.

No theory has so far been advanced to account for the regional character of secular variation here set forth, although both Bartels and Nippoldt have recognized its existence. Bartels finds that the regional element predominates over that belonging to the general field, while Nippoldt sees an important connection between magnetic secular-variation and the forces producing movements in the Earth's crust. The latter suggests that one may possibly conceive advance and retreat of heat as modifying magnetization or electric conductivity, but recognizes that the rapidity of the changes is far too great for the ordinary conceptions of geology or of the movement of matter to maintain isostatic compensation. Whatever the explanation, it is certain that these changes, which

are in reality very minute, are the most sensitive means of which we have knowledge for detecting the operation of forces within the Earth's interior. It is important, therefore, not only to the student of terrestrial magnetism but to those interested in other branches of geophysics, that we endeavor to secure the fullest record possible of magnetic variations and learn to interpret them as the surface-manifestations of deep-seated actions otherwise beyond our means of observation.

It is obvious there is much greater need of magnetic observatories and magnetic observations at frequent intervals in some regions than in others. It has been the purpose of this paper to emphasize the necessity of maintaining a vigorous activity particularly in those places where the need is great, and to call the attention of those working in other departments of geophysics once more to the very apparent importance of magnetic secular-variation to their investigations.

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EARTH-MOVEMENTS AND TERRESTRIAL-MAGNETIC VARIATIONS

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Long-period variations of the Earth's magnetic field and pseudo-magnetic variations accompanying earthquakes must be related in some manner to the conducting regions inside the Earth. In the present paper we will turn our attention to the interior of the Earth and investigate in a rough quantitative manner some of the electromagnetic reactions which appear to have bearing on terrestrial-magnetic phenomena. Very little is known about general earth-movements but it seems clear that the transport of material by large rivers from one part of the Earth to another will compress the interior layers where it is deposited and will permit a general expansion of the denuded region. If the core of the Earth is semi-liquid, it seems reasonable that such a phenomena may easily give rise to a general oscillation of long period which may be accentuated by tidal forces. Such motions are important in the geological history of the Earth and any mechanism which permits a study of them seems of interest.

In an earlier paper (Ross Gunn, Phys. Rev., v. 35, 107, 1930) it was pointed out that when a star having a magnetic field expanded or contracted, electrical currents were set up in the star's interior in such a direction that the interaction of the current with the impressed magnetic field produced a force which opposed the contraction or expansion. We shall see that when the change is rapid the forces produced during contraction are much larger than those produced when the body expands. A surprisingly small rate of contraction in a magnetic star produces current-systems which may double the surface-field.

The Earth is believed to be contracting very slowly and the continents are believed to be constantly readjusting themselves according to the requirements of isostasy. These readjustments seem capable of giving rise to current-systems which may modify the surface magnetic fields. Conversely, it appears that observations of terrestrial-magnetic variations over long periods of time may yield important information regarding earth-movements.

In order to get a quantitative estimate of the effects let us think of the Earth for a moment as a large (degenerate) conducting ring of self induct-

ance L and total resistance R. A current producing the Earth's permanent magnetic field flows in this ring as a result of the thermal motions of the conducting electrons moving about in an inhomogeneous magnetic field or in crossed electric and magnetic fields (Ross Gunn, Phys. Rev., v. 34, 335, 1929, and v. 34, 1621, 1929). Without entering into a discussion of the exact mechanism we can say that this interaction is equivalent to an electromotive force E impressed on the circuit which acts around the ring and produces a total current I. Let the Earth uniformly contract or expand; this changes its self-inductance. Lenz's law assures us that currents will be set up by the expansion which act in such a direction as to keep the total flux threading the circuit constant. That is, a contraction will increase the magnetic field both inside and outside the Earth and an expansion will decrease it. The circuital equation connecting the various quantities is

$$E/I = R + dL/dt + (L/I)(dI/dt) \tag{1}$$

where the symbols have the usual significance. The term in this equation involving the time-variation of the inductance has apparently been neglected heretofore since it has generally been assumed that the inductance is a constant. Numerically the changes are small but when (dL/dt) is negative (corresponding to a contraction) the resulting effects of a small change may be quite large. It will greatly simplify the problem if we assume (dL/dt) a constant and neglect the initial transient stage. When (dL/dt) is constant the solution of equation (1) is

$$I = E/[R + (dL/dt)_o][1 - \exp[-(R + (dL/dt)_o)/L]] \tag{2}$$

and the time for the current to reach approximately two-thirds of its final value or the time-constant τ is

$$L/[R + (dL/dt)_o] = \tau \tag{3}$$

if we call the initial current I_o then from equation (2) we have

$$I/I_o = 1/[1 + (1/R)(dL/dt)_o] \tag{4}$$

Thus the relative magnitudes of the magnetic fields depend on the ratio of $(dL/dt)_o$ to the total circuit resistance R. To a first approximation the self-inductance of a conductor-system is proportional to the circuit-area when the form is maintained. Thus if r is the radius of the Earth

$$(dL/dt) = (2L/r)(dr/dt) \tag{5}$$

This substituted in equation (4) gives a direct relation between the ratio of the currents (and hence the magnetic field) and the Earth's radial contraction or expansion.

The foregoing paragraphs discuss the effects of a general contraction of the Earth when the magnetic field contracts along with the body itself. In general such a contraction is of special interest only in relation to its bearing on the maintenance of the permanent magnetic field and of tidal deformations. It is of interest to calculate the magnitude of electrical currents set up inside the Earth as a result of a local movement which is restricted to a small fraction of the Earth's volume. Local movements such as the rise and fall of continents give rise to currents by the dynamo-action of the conducting region as it is moved across the Earth's fixed magnetic field. The following calculation made for a region near the magnetic equator assumes that the generated

currents do not greatly modify the impressed magnetic field; an assumption which is valid for ordinary terrestrial-magnetic variations. When a conducting region of the Earth sinks toward the center of the Earth under the assumed conditions, it will necessarily cut across the Earth's fixed magnetic field and by this action will produce an electromotive force. The electromotive force so produced is small but the resistance of the local circuit is also small and the resulting currents may be large. Suppose the section sinks with a mean velocity V and suppose the component of the Earth's magnetic field perpendicular to the motion is H, then the current I produced by the movement is

$$I = (HlV/R)[1 - \exp(- L_1 t/R)] \qquad (6)$$

where L_1 is the effective inductance of the circuit, R is the resistance of the local circuit and l is the length of the disturbed region measured in a direction at right-angles to both the motion and the magnetic field. The resistance of the entire circuit resides almost entirely in the moving region since the conducting area of the fixed region is much greater than that of the moving one and we may therefore neglect it. Moreover, when the region is not too large the effective self-inductance of the circuit is greatly reduced by transformer-action with the highly conducting material adjacent to the moving regions, so that it is usually legitimate to neglect the transient term. Thus if a is the width of the zone measured along the magnetic field, equation (6) becomes

$$I = a\, b\, H\, V/\rho \qquad (7)$$

where ρ is the specific resistivity and b the depth of the disturbed zone. By Ampere's circuital equation and equation (7) we can represent the disturbed magnetic field ΔH approximately by

$$\Delta H/H = 2bV/\rho \qquad (8)$$

if we consider the field only at a point near the current-sheet.

The resistivity of the conducting regions is an important factor in the relations which have been developed and it is necessary that proper values be selected for this constant in the calculations. Gish and Rooney (Terr. Mag., v. 33, 79, 1928) have made measurements of the resistivity at the surface of the Earth and found it to vary between 10^{13} and 10^{14} e.m.u., a value not greatly different from that obtained by Chapman (Phil. Trans., v. A218, 1, 1919) for the Earth's core. There is evidence that the Earth's core is very hot and Adams (Jour. Wash. Acad., v. 14, 459, 1924) calculates that even at a depth of 200 km the temperature has risen to 2,000° Centigrade. It is difficult to believe that the resistivity of the hot core of the Earth is of nearly the same magnitude as that actually observed at the surface. It is found that calculations making use of diurnal magnetic data to determine the resistivity of the core in the manner proposed by Schuster (Phil. Trans., v. A208, 163, 1908) and followed by Chapman are not allowable.

The diamagnetic theory (Ross Gunn, Phys. Rev., v. 32, 133, 1928) of the solar diurnal-variation of terrestrial magnetism has led to an explanation of the observed variations including the advance in phase of the horizontal component of the variation-field which was very troublesome of explanation in the older theories. On the new theory the maximum of the external variation-field falls correctly at about 11 o'clock local time. The horizontal component of the resultant magnetic variation-field is but lightly shifted by the presence of the conducting Earth, whereas the vertical component must be shifted about one hour later. The occurrence of the maximum vertical component one hour

after the external field reaches a maximum and the observed ratio of external field to resultant field are both readily explained by the presence of a uniformly conducting earth. (See, for example, Table K of the paper by Chapman, Phil. Trans., v. A218, 1, 1924.) Knowing the approximate inductance of the circuit; the angle of lag between the impressed and resultant field, the total resistance can be calculated and from this the resistivity. This incorrect method of calculation has been uniformly adopted in the past to determine the resistivity of the Earth's core. The trouble with the method is that it implicitly assumes the resistivity of the Earth's core is determined by the rotational period of the Earth and not by the physical properties of the conductor. What the method actually determines is the resistance of a surface skin whose effective depth is related to the frequency of the impressed variation-field. The important point is that, due to the electrical inertia of the Earth's core, magnetic fluctuations of a period approximating that of the Earth's rotational period would produce vanishingly small magnetic effects in the core proper if its resistivity is low. Thus the diurnal-variation data can not be used to determine the resistivity of the core proper. The present calculations show that observed variations having a period of at least a thousand years will be necessary to calculate the quantity accurately. If we assume that the Earth does not change size the time constant of the Earth's equivalent electrical circuit is $\tau = L/R$ where L is the inductance of the conducting sphere and R the total circuit-resistance. The inductance L is readily calculated from the geometry of the body and cannot be much in error. Its value is approximately 7.8×10^8 e.m.u. (Lamb, Phil. Trans., 1883). The observed angle of lag θ between the external diurnal-variation field and resultant field is about one hour or $\pi/12$ radians and is connected with the circuit time-factor τ by

$$\theta = 2\pi L/TR = 2\pi\tau/T$$

where T is the rotational period of the Earth. The relation is only an approximate one, for the magnetic variations are not strictly simple harmonic variations of a period T. Using this relation and taking $\theta = \pi/12$ we get $R = 2.2 \times 10^6$ e.m.u. This is the total resistance not of the core but of the electrical circuit lying in a surface-layer of unknown depth d. Geological evidence suggests that this surface-layer is about 200 km thick and it extends over a large part of the sunlit hemisphere. The cross-sectional area A of the circuit is approximately dr where r is the radius of the Earth and ℓ, the length of the current-circuit, is approximately πr. We calculate the resistivity ρ from

$$\rho = RA/\ell \fallingdotseq Rd/\pi$$

and find that $\rho = 1.38 \times 10^{12}$ e.m.u., a value in substantial agreement with that which would be expected from the work of Gish and Rooney. It seems probable that other distributions of the conducting layer are capable of giving the observed magnetic features.

We now turn our attention to the core and note that the mean temperature of the core is some thousands of degrees, almost certainly more than 3,000° and perhaps as high as 10,000°. At these temperatures the materials of the Earth are highly ionized and the resistivity must be several orders of magnitude less than that calculated for the surface-layers. No reliable estimate of the resistivity is yet possible even though several lines of evidence point to values lying between 10^8 and 10^4 e.m.u. By careful observation of secular magnetic-variations of long period and knowledge of actual mass-movements the value can be calculated from observation but this must remain for the future. It is important to note that even if the resistivity of the Earth's core were so large as 10^8 e.m.u. the time-constant of the core would approximate 100 years. It is clear, therefore, that variations due to the rotation of the

Earth having a period of a day will have a vanishingly small effect on the core because the external variation-field will be removed before the electrical circuit of the core can accommodate itself to the new condition.

We must consider the resistivity of the Earth's core an unknown quantity. Geologists seem to agree that the Earth is made up of three distinct regions: (a) A surface-layer extending downwards perhaps 200 to 400 km; (b) a very hot core of about half the radius of the Earth and largely composed of iron and nickel; (c) an intermediate region of molten rock.

The resistivity of the surface-layer we have just calculated. The resistivity of the other two regions must be calculated from the meager physical data available at the present time. We can estimate the resistivity of the iron and nickel core by a long-range extrapolation or we can calculate it from theoretical evidence deduced from the existence of the Earth's permanent magnetic field. The two methods lead to values which are not greatly different but we must recognize that both methods are dangerous. The resistivity of iron and nickel at ordinary temperatures approaches 10^4 e.m.u. while their temperature-coefficient approximates 0.006. Data regarding the change of resistivity of metals at their melting points indicate that the resistivity seldom doubles at the transition-temperature. Suppose the temperature of the iron core is so much as 6,000° then the resistivity of the material of the core turns out to be about 5×10^6 e.m.u. If the iron is not pure the temperature-coefficient is much less and it is not improbable that the resistivity is as low as the above figure suggests. A theory of the permanent magnetic field (Ross Gunn, Phys. Rev., v. 34, 335, 1929, and v. 34, 1621, 1929) of the Earth that attributes the field to electrical currents flowing in the hot core of the Earth (probably in the metallic core) requires that the electron mean free-path in the region approach 2×10^{-7} cm. Such a free-path leads to a value for the resistivity of 10^4 e.m.u. at a temperature of 6,000°. The estimates cannot be taken directly but they do indicate that the core of the Earth is highly conducting and that the resistivity is low. At the present time we can do no better than adopt a representative value for the resistivity, say, 10^6 e.m.u., and calculate the magnetic effects that may be expected if the Earth contracts slowly. First, we shall note that even if the time-constant of the Earth were so great as 1,000 years the decrease in the Earth's magnetic moment would be observed in a century or so and we are led to inquire if the contraction of the Earth might not explain the maintenance of the Earth's permanent magnetic field. A perfectly steady magnetic moment would be interpreted (if other mechanisms are neglected) to mean that the effective electrical resistance of the Earth were zero. We see from equation (1) that the effective resistance of the circuit can be zero if

$$R = -(dL/dt) = -(2L/r)(dr/dt)$$

If $\rho = 10^6$ e.m.u. we find $R = 3 \times 10^{-3}$ e.m.u. and, taking $L = 7.8 \times 10^8$ e.m.u. and $r = 7 \times 10^8$ cm, we get $dr/dt = -1.25 \times 10^{-3}$ cm/sec. That is to say, a general contraction of the Earth amounting to 0.00125 cm/sec would account for the maintenance of the Earth's permanent magnetic field. It is evident that such a contraction is unimportant in the history of the Earth's magnetic field for such a rate of contraction would reduce the Earth to half its present size in about 10,000 years. Even if the estimate of the resistivity were too high by two orders of magnitude the effect could not be of importance and we dismiss the possibility.

The principle of geologic isostasy requires that the continents continually readjust themselves due to changes in the loading and indeed compressional

movements due to atmospheric-pressure variations may not be inconsiderable. Readjustments in the poorly conducting surface-layers seem to be of small importance and we consider here only the movements of the deep conducting layers. The observed secular magnetic variations show that variations at the surface of the Earth may be as great as 10^{-3} of the total field. As an illustration, suppose that the Mississippi River carries great quantities of material to the Gulf of Mexico and deposits it there. This large deposit will compress the regions under the Gulf moving them downward at a very small velocity and giving rise to electrical current-systems which in turn will increase or decrease the surface magnetic field according to the assumptions made in regard to the distribution of the current-systems that maintain the Earth's permanent magnetism. This downward displacement and the removal of material from the Mississippi valley will allow the valley to rise which will produce a magnetic variation of opposite sign. Were the process a strictly continuous one there would be very little magnetic change but it is not improbable that the movements are variable and perhaps periodic. Suppose that in the above example $\Delta H/H$ is taken as 10^{-3}. Then if we take $b = 5 \times 10^8$ cm, $\rho = 10^6$ e.m.u., and use equation (8) we find that the required velocity of movement to produce the effects must approximate 10^{-6} cm/sec or 30 centimeters per year. This may be too large but if the estimate of the resistivity which has been selected is too high the required velocity will be reduced. The evidence for such movements and magnetic effect in the light of these calculations seem inconclusive but it does appear to offer interesting possibilities. A calculation essentially similar to the foregoing shows that local earth-movements of the type encountered in earthquakes are incapable of producing world-wide disturbances unless the movement extends well into the conducting region below 200 km. The disturbing magnetic field as calculated from equation (8) using the surface value for the resistivity is small for shifts of the order of a meter, such as might be encountered in earthquakes. Any appreciable magnetic disturbance produced would be confined to the general region of the earth-movement. Available data show that the usual earthquake is limited to the Earth's crust and the present considerations are in accord with the known fact that general magnetic disturbances and ordinary earthquakes are not connected. The calculations do indicate, however, that a purely local magnetic disturbance may be expected for a surface movement or a general disturbance produced if the movement extends to the deeper layers.

The body tides raised on the Earth by the moon amount to about 25 cm and it seems likely that the tidal deformation of the Earth changes its self-inductance periodically in such a manner as to produce magnetic effects. These effects must necessarily be important in the surface layers where the time-constant approximates the impressed tidal period. The lunar component of the diurnal magnetic variation is very small and appears to be capable of qualitative explanation by Body tides, especially if account is taken of the fact that the magnetic axis does not coincide with the geographic axis.

The foregoing paragraphs hardly serve as an introduction to all the possible magnetic effects but they should be sufficient to indicate that there may be a direct connection between earth-movements and terrestrial-magnetic variations. Definite quantitative agreement awaits the accurate determination of the conductivity of the core either by experimental methods or on theoretical grounds. It seems certain that earlier estimates of the conductivity of the core are greatly in error.

U. S. Naval Research Laboratory,
Washington, D. C.

SECTION OF OCEANOGRAPHY

(Washington, D. C., first session, May 1, 1930)

THE PROGRESS OF THE WOODS HOLE OCEANOGRAPHIC INSTITUTION

H. B. Bigelow

I suppose that the establishment of the Woods Hole Oceanographic Institution has been pretty well advertised; at any rate we have had an announcement in "Science" to that effect and we are very glad that the development of the plan has been rapid enough to justify us in giving you a report of progress already.

It is easy to see, now that we can look backward, that affairs had been shaping up for several years towards such an institution, but I think those of us who have actually been actors in the piece so far are a bit bewildered that the thing, like a snowball, has grown so fast. Probably most of the members of the Section know of the successive steps--first came the request of the Rockefeller Foundation to the National Academy of Sciences for information as to the status and present needs of the science of oceanography; then the presentation by a committee of the Academy of rather a long report on that subject in general; the acceptance of this report by the parent body last November; the conclusion of the committee that the most immediate need at the moment was the establishment of a new institution on the Atlantic coast to encourage the study of the sea in the broadest way; and following all that the almost immediate response of the Rockefeller Foundation through their decision to endow such an institution. The consequence of all this was the establishment of the Woods Hole Oceanographic Institution as a Massachusetts Corporation.

We may regard as its birthday the first meeting of its Trustees which fell on January 15, 1930. The first Board of Directors (we call them Trustees) includes the members of the National Academy Committee on Oceanography with five others--the complete list is given in the announcement in "Science." The by-laws provide for an increase from this initial thirteen to a total of twenty-four, giving an opportunity to enlist the support of the various interests that seem likely to be concerned with the whole project.

Perhaps you will be interested in the financial set-up, for this has not previously been published. The Institution has a million dollars for construction of building, ship, docks, etc., for equipment, and for maintenance. Then for scientific operation, there is an endowment of a million, plus an additional grant of $50,000 a year for ten years. So for these initial ten years the income for scientific operation is $100,000 a year. One can naturally presume that what will happen after ten years are past will depend on how thoroughly the Institution has justified itself.

To complete a statement of progress to date, I may remark that plans are nearly complete for a laboratory building on the water-front at Woods Hole to accommodate about fifty investigators, with facilities for all the generally recognized branches of oceanographic work; that is to say, with a room containing aquaria, with arrangements for experiments under externally controlled temperature for the oceanographic biologist, with a chemical laboratory, with work-rooms for dynamic, meteorologic, and other physical studies; with space for the study of sedimentation; and with reading-rooms, storerooms, shops, etc. We hope to start actual construction on the building early in the summer. This I myself regard as the least interesting part of the pro-

gram, for bricks and mortar by themselves bring nothing to pass.

A far more interesting part of the set-up is that the Institution is to own and operate a sea-going ship, fit to go anywhere at any time on the high seas, and able to carry on any of the kinds of oceanographic work that are now to the fore. And when one really gets back to the heart of things, it is this ability to have such a ship, that is, to get out upon the sea, that should be made the keynote of the whole undertaking, for if we could not provide for this, the Institution would seem to me perilously like an astronomical observatory without a telescope. This seems so obvious to me that it is useless to waste your time trying to explain the whys and wherefores.

As we are talking now about practical details, you may be interested in a word as to the type of ship that we have settled upon. We are ready enough to agree that this is the age of power and of speed and all that sort of thing. But here is one of those cases where speed does not count--where speed may even be a nuisance. Usually when one builds a ship, one does it to get somewhere as quickly as possible. But the problem in our case is not to go somewhere but to be able to stay still when you get there and to do your work with ease, neatness, and despatch. So what an oceanographic ship really needs, next to safety, is steadiness and then more steadiness. No ship has ever been steady enough. And the next thing you need is ability to hold your position, that is to say, the ship must be so designed that she will lie-to well and not drift off sideways with the wind like a crab. The third requirement is comfort. For while any oceanographer ought to be able to take the rough with the smooth, we regard comfort as a prime necessity for this reason.

Unless the scientist will go out on our ship and have a pleasant time, and unless all hands come to look on a cruise as a joy, not as a penance, the right people will not join in and the thing will not go. And that emphatically means that we do not want a beam-trawler or a motor-yacht. They roll and toss and bounce too much. And then the ship must be small enough for us to operate within our means and yet enough of a ship to go to sea and keep working, blow high or blow low.

The general outcome is that the only suitable type is a combination of sail with power, about equally proportioned, with the hull more of the sailing ship type. And I am happy to state that one of the most experienced of European oceanographers, whom we are glad to have with us here now, backs us in this decision.

So we are planning on a ship about 140 feet long, with 250 to 300 horsepower Diesel engine, with a good sail-spread so we can turn off the engine when we want to, with quarters for five or six scientists in luxury and for half a dozen youngsters if packed in close. Our hoisting machinery is an interesting problem because we must be able to trawl as well as to use a lighter gear, and to work down to any depth. We think that we have this well worked out, so I will not bother you with details here.

Now we come to the real kernel of the nut--when we get all this fine equipment, what are we to do with it? And of course it is on how this problem is met that success or failure of the undertaking hangs. In facing this problem, we will have to bear in mind that the stated purpose of the Institution is first to encourage the study of the sea. And we harp on this word "encourage" because we will all agree that the problems of the sea are so complex and are spread over so vast an area that no one institution by its own efforts and through its own staff can make anything like the progress that would follow

from the concerted efforts of various agents. But at the same time, we feel very sure that one of the best ways to encourage sea science, or any other science, is by precept. That is to say, it is the duty of the institution itself to start and carry on a program of research so attractive intellectually that others will naturally be drawn in. Thus one of the three major tasks will be to start and to carry on continuous investigations of the part of the sea that we can most easily reach, following a definite program. It is too early yet to state just what this initial program should be; our chief task during the next six months is to work out the plan for this. And obviously the details will depend largely on who is associated with the Institution, all of which has yet to be worked out. After all, we must remember that the whole affair is only four months old and that like any other baby it needs time to grow up.

But in a general way, it is clear enough that the unavoidable problem in any center of sea-science located in this part of the world is what we may call the problem of the north Atlantic--that is, why is the north Atlantic as it is? And in our case, this naturally narrows down in the beginning to the problem of the northwestern Atlantic. Fortunately for the general situation we can fairly say that this is the most interesting part of the Atlantic, for the whole range of phenomena from extreme to extreme, whether geophysical or biological is crowded into a smaller space there than anywhere else in the ocean, comparatively close to our base of action.

As a beginning then, perhaps we can think of periodic cruises to cover a definite rectangular area bounded by a line running from Cape May to Bermuda, then to a point some 700 to 800 miles northeast of Bermuda, and finally into Nova Scotia; these quarterly cruises must be sufficiently synchronized with those carried out by the International Council on the other side of the Atlantic. On such cruises, the work should be both biological and physical-chemical, with the emphasis naturally varying from time to time according to what particular fields are seemingly most fertile. We may think of more intensive work in smaller areas on one problem or another and we should point out that we hope to develop an extensive attack upon some of the puzzling sides of submarine sedimentation.

We must remember, too, that affairs in the sea do not stop in a bad season so we must look forward to working in winter as well as in summer. And there will be nothing to stop us from going out on much longer cruises, if this seems the thing to do, from time to time.

I have been asked a good many times how much we were going into the arctic and how much into the tropics; it is too early to answer yet but we must be able to do either to a reasonable extent.

In a general way we can say that the station-program at sea is part of a three-fold task. A second part is to secure cooperation of universities and to encourage them to back the study of oceanography to a greater extent than is now done. Just how this will work out would lead again into the realm of prophecy but it seems certain that for this sort of cooperation to prosper, it will not be enough simply to offer laboratory facilities to visiting students, as the seaside biological laboratories do, but it will be necessary to arrange for collaborators from the universities to take an active part in the program of research that the Institution itself is carrying on. We must recognize that oceanography cannot be attacked successfully in as individualistic a way as can marine biology, simply because most of the raw data for every problem must be obtained from a boat, large or small, and a boat can only be in one place at a time, and can only do a limited number of things simultaneously. In sea-

science, different lines of work have therefore to be coordinated and oceanographers are also beginning to feel an increasing need for intellectual coordination. So in short, the station-program will have to be more or less directed. But nobody realizes more keenly than we that if this direction is not sufficiently elastic, the whole thing will suffer from a sort of mental hardening and crystallization that will be fatal to any real accomplishment. Therefore, we must offer to visiting oceanographers every faculty we have--throw open the doors wide--but at the same time try to prevail on them to help in our general program.

The third duty, as we now see it, is to make things easier for students who are attracted to the study of the sea, but who at present can find no very good way to follow that inclination. There is no need of enumerating the obstacles which at present confront a young student who wants to become an oceanographer. Even with the new institution, we will have difficulty smoothing out all of these difficulties. It is necessary for the universities to do their part by offering more concrete courses of instruction in oceanography as a definite part of physics, geology, and biology. But there is one thing we can do, and that at present no one else in America can--we can provide opportunity for students who have the proper background to learn oceanographic field methods at first hand. This will mean taking parties of students out to sea on the cruises and having them lend a hand in everything that is going on, so that they can not only experience the joys of life at sea but can have an intimate contact with the ocean that will perhaps encourage them in their studies. For it seems after all that there are still young men coming along in America who are interested in the sea and its wonders and who (I almost said mirabile dictu) are no more dismayed at the prospects of going to sea and hauling on a sheet or hallyard, or of putting their backs into getting a loaded trawl aboard, than they are at the purely studying end of their scientific labors.

And that is about how the situation stands today.

Museum of Comparative Zoology,
Cambridge, Mass.

PARTICIPATION OF THE U. S. NAVY IN OCEANOGRAPHY

C. S. Kempff

The speaker found in preparing for this address that the selection of his subject, that is, the participation of the United States Navy in oceanography, was such a broad one that he really had assumed a task too great to cover the field in any detail within the time allotted. As the Navy has participated in oceanography for almost 150 years, it was decided that there should be presented a brief background and the rest of the time will be allotted to a discussion of present and future plans of the Navy in the oceanographic field.

Names of Naval officers associated with enduring accomplishments are Wilkes in the antarctic, whose surveys join up with and are associated with the present work of Rear Admiral Byrd; Matthew Fontaine Maury, whose oceanographic studies, investigations, and works are the bases of the pilot charts covering the oceans of the world; Admiral Peary, the discoverer of the North Pole; Tanner, commander of the Albatross and inventor of the Tanner sounding-machine, whose work in the field of ocean-deeps is a permanent and outstanding contribution in the field of configuration of the ocean-basins; Commander Reid, who first traversed the Atlantic by airplane, a pioneer in transoceanic air-

navigation. This last accomplishment logically led to the production of the pilot charts of the upper air, which, though valuable now to aviators, are becoming more and more valuable as they develop with the application of informa- obtained from transoceanic and overseas flights and from other domestic and foreign sources. The brilliant exploits of Rear Admiral Byrd in flying over both the North Pole and later the South Pole, coupled with his explorations and accumulation of invaluable data, called for furnishing of special charts and navigational data from the Hydrographic Office. To mention only the South Pole Flight, special navigation tables were prepared, enabling the air-navigator to carry out continuous determinations of his position as far south as the Pole. In addition, special mercator projections were requested to enable this explorer to fly as close to the Pole as practical. The construction of the projection was a simple matter, but it may be noted that the Mercator projection was carried to a latitude of 89° 45' south, only fifteen minutes of latitude from the Pole. With a plane flying at 120 miles an hour, he would have travelled two miles a minute, or would reach the Pole in seven and one-half minutes after flying off of the chart. However, to cover this interval, polar charts filled in the gap. As is well known, if the Mercator projection were carried to 90° latitude, the chart would have to be infinitely long, and as the Hydrographic Office did not have chart-paper of that length and the plane could not carry a piece of paper infinitely long, it was necessary to cease the projection at 89° 45'.

It may be mentioned that the participation and explorations of the Navy in these virgin fields, the production of original charts, sailing directions, and the collection of data, has blazed the trail followed later by other scientific expeditions. In a field so vast all contributions of this nature are not without value.

The recent circumnavigation of the world by the Graf Zeppelin afforded an opportunity to the Navy to assist Captain Eckner with charts, pilot charts, navigation books, routings, and other material. The response from both the commander and others on the expedition is most gratifying. The value of this association has not ceased and future contributions from that source may be expected.

The speaker is deeply impressed with the resolutions just passed by the Section of Oceanography relating to that distinguished scientist, seaman, and navigator, Captain Ault, a Lieutenant Commander in the United States Naval Reserve. The Navy profoundly mourns the premature ending of this distinguished career and appreciates the privilege it had in assisting the scientific work by furnishing material to aid in its accomplishments.

At a conference this morning with the Secretary of the Navy, Mr. Charles Francis Adams, I am authorized to state that he expressed a profound interest in the replacement of the Carnegie. Whether by that he means that the Navy should replace the ship or that he appreciates deeply the value of the magnetic and oceanographic work done by that vessel, I cannot at this time announce. It is pointed out, however, that the charts of magnetic variation, horizontal force, and vertical force are compiled for periods of each five years by the Hydrographic Office and, though formulae enable these magnetic forces to be predicted, it is highly necessary and important for the magnetic surveys of the Earth to be continued constantly and that these predictions be checked. Otherwise in time, false factors may creep in.

Coming to the present and future plans for the Navy in participation in the field of oceanography, the interest of the Service is shown by the action

of former Secretary Curtis D. Wilbur in organizing an oceanographic board with Rear Admiral Frank H. Schofield, Captain Charles S. Freeman (Superintendent of the United States Naval Observatory), and the speaker as members. Dr. Henry B. Bigelow of Harvard University displayed a great and effective interest in this work and pointed out the need of this program. His cooperation and his contribution of broad, accurate, and profound scientific knowledge did much to bring this policy into existence.

I will not take the time to cover the details of the work done by the Board, but will read its recommendations:

"(1) The preparation and issue to the service of a world-chart, showing unsurveyed areas of the ocean.

"(2) That instructions be issued to commanders-in-chief and to vessels operating singly providing that vessels suitably equipped for sounding operations take soundings in unsurveyed areas whenever their operations permit; that these soundings be transmitted promptly to the Hydrographic Office of the Navy Department.

"(3) That in addition the Department indicate, when circumstances permit, to vessels making passage at sea, a particular line of soundings which the Department desires made.

"The preceding recommendations are based upon the idea of a systematized and coordinated effort in developing contours of the ocean-bottom.

"(4) That a standard thermometer for the taking of ocean-temperatures, both of the air and of the surface-water, be adopted, and that suitable instructions be issued by the Hydrographic Office standardizing methods of taking and recording observations and of transmitting these to the Hydrographic Office; that these instructions include the methods of standardizing the instruments from time to time and of data sheets furnished the Hydrographic Office, the last time of standardization, and any comments that appear appropriate regarding the reliability of the data submitted.

"(5) That the Department express to the National Academy of Sciences its willingness to cooperate in obtaining samples of the surface-water of the sea by vessels in transit; that it suggest to the National Academy of Sciences the desirability of standardized containers of small size for this water with labels in suitable form for the recording on those labels of identifying data.

"(6) That when the nature of naval operations permit, suitably equipped vessels operating in the vicinity of ocean-deeps be directed to make a survey of these deeps, that they be directed to do so, and that a standardized plan be prepared by the Hydrographic Office for the conduoting of these special examination of deeps, so that the data furnished will meet the requirements of those scientists specially interested in ocean deeps.

"In conclusion, the Board stresses its opinion that it is highly desirable for the Navy to undertake the class of work above indicated. While the direct and immediate contributions to oceanographic knowledge may not appear to be of great practical use, the Board is convinced that the possibilities involved in the accumulation of the data outlined may

result in valuable conclusions permitting weather, current, and other predictions of sufficient importance fully to justify the effort."

These recommendations were approved by Admiral Charles F. Hughes, Chief of Naval Operations and by Charles Francis Adams, Secretary of the Navy, and this program is now in operation. It is regretted that the pressure of work and a certain limitation in funds does not give us the speed desired, but much has been and is being accomplished. The program has been carried out already to the following extent: The Hydrographic Office has prepared twenty-six large charts covering the surface of the world on which are shown the present known ocean-contours and ocean-deeps; using these given charts as bases, charts showing areas in which soundings are needed are being prepared and will be furnished to all Naval vessels so that sonic depths will be obtained by these vessels when passing through such areas. Steps are being taken for the proper standardization of thermometers and the adoption of proper receptacles for water-samples for the collection of salinity-data.

One of the most recent accomplishments is a complete contour of the Pacific from the Pacific coast to Japan, obtained by the U.S.S. Ramapo. Contours from the Pacific Coast to Australia, New Zealand, and Samoa by the U.S.S. Milwaukee and U.S.S. Cincinnati, as well as numerous other contours have been obtained by vessels of the Fleet equipped with the sonic depth-finder. The Hydrographic Office is undertaking to publish these ocean-deeps in pamphlet form periodically for the use of scientists.

Turning now to the subject of ocean-deeps, the value of a thorough investigation of these as a matter of oceanographic importance is appreciated. The Navy proposes that an accurate development of the ocean-deep north of Porto Rico be undertaken. It is considered that a thorough examination of one of these deeps will be a real contribution and really more valuable than partial or incomplete examination of a larger number. The survey vessels proceeding to Cuba and Panama, namely, the U.S.S. Hannibal and U.S.S. Niagara, are available for the development of this area. It is planned to obtain lines of soundings spaced at one-mile intervals at right-angles to a line marking the major axis of this deep and at such short intervals as will develop fault scarps, if such exist. Salinity-data, water-samples, and bottom-samples will also be procured. It is regretted that time will not permit the covering of such fields as the ice patrol or the recent development of the Guardian Bank off the coast of Costa Rica, where previously unknown shoal water was discovered by the Destroyer Litchfield and the U.S.S. Lexington while approaching Panama this spring. It is intended that the configuration of this area be determined.

The speaker desires to thank each and all for their attention to his address given from notes instead of his reading a prepared paper, his understanding being that the address would be of an informal nature.

U. S. Hydrographic Office,
Washington, D. C.

ECHO-SOUNDING

R. S. Patton

The science of oceanography is so broad that one who is asked to give a paper on some aspect of that science, and himself to choose that aspect, may well find himself embarrassed by the wealth and diversity of the subjects among which he must choose. I consider myself particularly fortunate therefore that this privilege comes to me at a time when at least from the viewpoint of the hydrographer, one aspect of the subject stands out preeminent.

I refer to echo-sounding. Accurate and complete charts showing the depths of the oceans and the configuration of their bottoms are important prerequisites to progress in any branch of oceanography. In consequence any method which facilitates the production of such charts is of interest alike to the geophysicist, the geochemist, and the biologist.

You all know what echo-sounding is. Perhaps to be strictly accurate, I should say that you all know what it was a few months or a couple of years ago. Because echo-sounding is still in the developmental stage, and its evolution has been so rapid that it is by no means the same thing today that it was even so recently as a year ago. But I assume that you do have that general knowledge which is sufficient for my present purpose, which is to discuss, not the thing itself, but rather, its application to oceanography; the extent to which it lends itself to an enhancement of our knowledge of the sea, and the limitations to which its use for that purpose is subject.

During recent years echo-sounding may be said to have been the favorite child of the hydrographic services of the principal maritime nations, and some of them have manifested some of the tendency so commonly observed among fond parents, of extolling the virtues and passing lightly over the defects of their offspring. For that reason, and because those virtues are so obvious to every one, I shall dwell on them but briefly and give the greater part of my time to commenting on some of the limitations to which the method is subject.

A reasonably accurate and complete chart of the oceans has long been ardently desired by oceanographers. Such a chart is a first essential to the solution of a large percentage of the problems with which the science is concerned. Even in those cases where the data it contains are not directly involved in the study of such problems it affords a background against which the pertinent factors can be arrayed, appraised, and correlated better than by any other method.

Although man has been sailing the seas for centuries, the task of producing this world chart is only begun. The various maritime nations have more or less adequately surveyed the narrow shelves bordering their respective coasts, but even within these very limited areas much of the work is not up to the standard set by modern requirements. Beyond these narrow fringes great expanses of the oceans lie virgin before us. Here and there the result of exploring expeditions, or of cable surveys, have made a small beginning, but on the whole it may be said that the amount thus far accomplished is negligible in comparison with that which remains to be done. In the northern Pacific region there is an area half the size of the continental United States in which not a single sounding has been recorded.

The development of echo-sounding for the first time brings the production of this world-chart within the relam of feasibility. By this method a vessel

proceeding at full speed about her regular business can have instant and constant knowledge of the approximate depths beneath her. The method therefore has a potential value far exceeding that resulting from the increased accomplishment possible to the occasional exploratory expedition. It makes possible an effective cooperation by both naval and merchant ships; a cooperation limited chiefly by two factors, first, the will to participate, and second, and more important, the fact that shipping does not traverse systematically all the waters of the oceans, but instead adheres closely to a limited number of tracks leading from port to port. In spite of these limitations I believe we must look to a cooperative effort of this character for our first effective progress, possibly to be supplemented in due time by special expeditions to sound those areas into which the regular tracks did not penetrate.

Because of the alluring possibilities suggested by this brief general picture of echo-sounding, it becomes important to ascertain the extent, if any, to which defects or limitations in the method detract from its value. The principal purpose of this paper is to discuss certain problems involved in the answer to that question.

The principal limitations with which we are concerned are two in number. First, in echo-sounding we measure the time required for a sound-wave originating at the ship to travel to the bottom and be reflected back to the ship. The method therefore is based on the velocity of sound in sea-water. It is well known that this velocity varies with the temperature, density, and salinity of the water. The maximum possible range of velocity from all factors combined is quite considerable, ranging from 765 fathoms per second in cold fresh-water to 890 fathoms per second at the bottom in the greatest known depth of the oceans. Thus all echo-soundings, which are taken by instruments calibrated for assumed average values for these variables, must be corrected to conform to the actual values ascertained to exist at the point where the sounding is taken. Stated in other terms, the errors which would otherwise result are: Assuming correct values of salinity and pressure, an error of one degree Centigrade in the temperature will produce an average error of about two per cent in the depth; assuming correct temperature and pressure, an error in the salinity of one part in a thousand results in an error of approximately 0.4 per cent in depth. With the mean temperature approximately known, there need be no error in the adiabatic (pressure) correction.

Within the areas investigated by the Coast and Geodetic Survey considerable ranges in these factors, particularly in the temperature, have been found. For example, the Survey steamer Guide, on an oceanographic cruise from New London, Connecticut, to San Diego, California, encountered a temperature-range of 28° Centigrade and a salinity-range of 5.5 parts, in depths ranging from 185 to 4,617 fathoms, necessitating the use of computed velocities ranging from 810 to 841 fathoms per second.

Temperature studies in the Hawaiian Islands, off the coast of Lower California, the coast of Oregon, and in the Gulf of Alaska, respectively, confirm the generally accepted opinion that in the great deeps of the ocean the variations in temperature are confined principally to a shallow layer near the surface. In these four localities, at a depth of 100 fathoms the range is 13°, at 200 fathoms it is 5°, and at 450 fathoms it is reduced to 1°.

It is therefore on and adjacent to the continental shelves that a detailed knowledge of the temperature-ranges is particularly essential to results having a small percentage of error. In its own surveys, which are practically confined to such areas, the Coast and Geodetic Survey determines the temperature-

corrections to be applied to the echo-soundings taken in each area surveyed. For this purpose it measures the temperature at a series of depths, beginning with 6-fathom intervals from the surface. This interval is gradually increased, subject, however, to the limitation that if the difference between any two consecutive measures exceeds 1°, an intermediate measurement must be made. By this method, applied at a number of points throughout the area to be surveyed, the mean temperatures within any range of depths are ascertained to within a small fraction of a degree. These measurements are made at an early stage of the survey work. Thereafter similar measurements are made from time to time to insure that there shall be no undetected change due to seasonal or other causes.

Simultaneously with these temperature-determinations, the salinity is measured with a comparable accuracy.

It does not appear that the limitations imposed on the echo-sounding method by the necessity of correcting for these factors is such as to cause us any grave concern. Undoubtedly the program of any exploring expedition which took echo-soundings would include observations of temperature and salinity. There would remain the problem of securing data to correct soundings taken under a cooperative plan by naval and merchant ships which could not be expected to interrupt their voyages to observe temperatures and salinities. For some areas, particularly in the Atlantic Ocean, we already have data probably sufficient to provide correction-factors of substantial accuracy. In other areas additional determinations would be required. There seems little occasion to doubt, however, that these could be available by the time they were required for the correction of any considerable accumulation of echo-soundings.

The second limitation to which reference has been made results from the facts (1) that echo-sounding measures the distance (called, for convenience, the echo-distance) from the ship to the nearest point on the bottom, and (2) that this nearest point is vertically beneath the ship only where the bottom is horizontal. In other words, if the bottom be sloping the echo-distance is not the depth, and the relationbetween the two may or may not be susceptible of determination.

Uncorrected echo-distance will give an approximate indication of the extent and character of the slope. Where the slope is uniform it is a simple matter to transform an echo-distance into a depth by applying the relation depth equals echo-distance divided by the cosine of the slope. Where, however, as in the case of the Coast and Geodetic Survey, many thousands of echo-distances are measured each year, it becomes such a laborious and time-consuming task to correct each one that the imperative need for maximum accomplishment with limited facilities compels all possible reduction of that task within the limit of the necessary degree of accuracy in the final product.

The Coast and Geodetic Survey has reached the conclusion that within certain clearly recognized and carefully observed limits of depths of water and degree of slope, echo-distances need not be corrected for slope in order to merit inclusion on an accurate chart. The conclusion will be better comprehended if, before stating the limits, we follow the steps by which the conclusion was reached.

For every practical purpose a sounding must be considered as having three dimensions. A measurement of depth is of value, not simply in proportion to the accuracy with which it was made, but equally in proportion to the accuracy with which we locate the point on the ocean's surface from which it was made.

A group of accurate soundings plotted on the chart in their correct positions relative to each other will give an accurate indication of the form of a complicated area of the ocean-floor. The same soundings, incorrectly placed with reference to each other may give a seriously erroneous picture.

Therefore, while it is proper to think of the uncertainties of echo-sounding over a sloping bottom in terms of the difference between the echo-distance obtained at any point and the depth at that point, it is equally proper to consider them in terms of the displacement; that is, the horizontal distance from the point at which the echo-distance was measured, to the point at which that distance is equal to the depth.

For the practical purpose of chart-production and use, it is the latter relationship which is significant, because of the variety of scales on which charts are constructed. Thus, on a scale of 1/1,000,000, each digit of the number in which the sounding is stated occupies an area equivalent to more than a square mile on the Earth's surface. Obviously, in compiling such a chart, displacements of several hundred feet can be ignored without detracting from the practical value of the product.

The "hydrographic sheet" is the permanent record of the field survey. On it soundings are plotted in position. From it the charts are compiled by selecting, from the multitude of soundings available, the relatively small number of characteristic ones which give an adequate and correct representation of the configuration of the bottom. It is always plotted on a much larger scale (usually at least double) than any chart to be compiled from it.

It is in the plotting of this sheet that the slope-corrections must be taken into account, if at all. The rule adopted is "Begin correcting for slope when the displacement on any scale in inches equals the width or twice the heigh of a sounding figure in inches." To determine the slopes, echo-distances are plotted as soundings and preliminary depth-curves are drawn. The slopes are derived from the distances between successive curves.

It is important to note that a satisfactory determination of slope demands a systematic survey. There is no assurance that a slope derived from any single line of soundings is the true slope.

The foregoing is based on the assumption that the slopes are extensive and substantially uniform. In areas of rugged relief the present echo-sounding method may fail entirely. In such a region the vessel will obtain a series of echo-distances to consecutive salients and few or none from the intervening depressions, and the profile derived from any slope-corrections which can be applied will be wholly fictitious.

We know so little about the actual form of the ocean-floor that we cannot appraise the seriousness of this limitation. Obviously the charting of a submerged mountainous region presents a problem which has not yet been solved. Some hope for that solution lies in the possibility of utilizing a directional super-sonic wave which would measure the echo-distance vertically beneath the ship. As a last resort we always have the possibility of supplementing our echo-sounding with wire-measurements.

On the other side of the picture, we have no definite assurance that any considerable part of the ocean-floor is so rugged that the echo-sounding method would fail. The only feasible way to learn the facts is by the use of echo-sounding. This uncertainty, therefore, is but an added argument for an early beginning of the task of producing our world-chart.

To summarize, in view of the urgent need for this chart of the oceans, of the time required for the collection of the necessary data, of the demonstrated feasibility of using echo-sounding as a major method of obtaining such data, and of the extent to which world shipping is now equipped with echo-sounding devices, I believe that the time is ripe for a beginning of the project.

U. S. Coast and Geodetic Survey,
Washington, D. C.

RECENT OCEANOGRAPHIC RESEARCH AT THE SCRIPPS INSTITUTION OF OCEANOGRAPHY, UNIVERSITY OF CALIFORNIA

Thomas Wayland Vaughan

The general program of research at the Scripps Institution has been presented on so many occasions that there seems to be no need to repeat it. It will be said, however, that the first endeavor in the plan of the Institution's work is to get as good a picture as is practicable of the physical and chemical features of the masses of water with which we are dealing and that the next endeavor is to relate the results of the study of other marine phenomena and processes to that general setting. The following synopsis will present the recent results of the researches under four captions: (1) Dynamical oceanography, (2) chemistry of sea-water, (3) biology of marine organisms, (4) geologic processes.

Dynamical oceanography

Publications--During the past year George F. McEwen has published three important papers. The first is "A mathematical theory of the vertical distribution of temperature and salinity in water under the action of radiation, conduction, evaporation, and mixing due to the resulting convection" (Scripps Institution of Oceanography Bull., Tech. Ser., v. 2, No. 6). This paper presents a mathematical formulation and solution of the problem of the downward diffusion of the surface cooling due to evaporation and back radiation and of the heating caused by the absorption of solar radiation. The method provides a means of estimating the rate of evaporation from the surface, the rate at which solar radiation penetrates the surface, and the rate of upwelling. The requisite observational data are air-temperature and humidity, and serial temperatures and salinities made at time-intervals of about two weeks. Formulae, tables, and numerical solutions based upon actual field data are presented to facilitate the application of the method.

The two other papers are "The water-cycle between the ocean, atmosphere, and land" (University of Southern California Short Course, Water Supply Section, School of Citizenship and Public Administration, pp. 5-17, 1929) and "A limnological study of lakes and reservoirs of San Diego County-Stagnation and overturning in relation to vertical temperature-distribution" (Water Supply Section, School of Citizenship and Public Administration, April 1930).

Computations of oceanic circulation--By arrangement with Dr. W. F. Thompson, director of the International Fisheries Commission, McEwen has made computations of the oceanic circulation relative to the bottom at a series of depths in the Gulf of Alaska. There were three sections perpendicular to the coast between Ocean Cape and Cape Chiniak averaging 400 kilometers in length. Serial observations were made at 41 stations. A prevailing westward drift of about one-half knot was thus indicated.

Lapse-rates--Evaporation-investigations at the end of the Scripps Institution's pier, made by N. W. Cummings and Burt Richardson, involved measurements of temperature, humidity, evaporation, and water-temperatures at three levels--near the water-surface, on the pier deck 30 feet above sea-level, and at a height of 60 feet. Very small differences were found in measurements made at various levels, and a very close agreement was found between the solar radiation estimated from observations at each pan and the indications of an Englehard instrument recording solar radiation at a level of 60 feet.

Chemistry of Sea-Water

The relation of diatoms to physical and chemical conditions of the water--This work was done by Moberg. From Professor Allen's diatom-counts for various levels at a station located ten miles from shore the average vertical-distribution of the diatoms during the season was calculated. This distribution was compared to the average distribution of temperature, density, salinity, pH, CO_2, NO_3, PO_4, and SiO_2 for the same period. In the case of the chemical substances there was a decided increase in quantity between 25 and 50 meters and at these depths there was also a change in the physical properties of the water. At 30 and 35 meters the maximum number of diatoms was found, whereas above 30 meters, where the concentration of the nutrient substances was low, the diatoms were very scarce. In the upper 15 meters of water nitrate was virtually absent and it is probable that lack of this substance limited the growth of diatoms. From studies made on the penetration of light in sea-water in other localities it appears probable that diatom-growth at lower levels where nutrients were abundant was prevented by insufficient illumination. These studies emphasize the importance of upwelling subsurface water, which is rich in chemicals, upon the production of phytoplankton.

Studies of vertical distribution of nutrient substances were also carried out in Monterey Bay by Miss Leslie in connection with a survey of this body of water made by Dr. H. B. Bigelow. In this case, too, it was found that the surface-layer is supplied with chemicals by upwelling subsurface water.

The oxygen-content of the water in the Pacific--At the Scripps Institution pier the quantity of oxygen in the water was determined daily for about two years by Miss Leslie. The oxygen-content was at a point of supersaturation during most of the year and showed a very close relation to the amount of temperature reduction which is correlated with the intensity of upwelling.

On board the Carnegie between San Francisco and Honolulu oxygen-determinations were made by Moberg on 190 samples collected at ten stations. At the surface the water was practically saturated with oxygen but with increasing depth the quantity decreased to a minimum at 700 meters below which there was slow but uniform increase toward the bottom. The minimum oxygen values ranged from 0.2 to 1.0 ml per liter which is considerably lower than the minimum at similar latitudes in the Atlantic. Below the minimum layer the highest quantity of oxygen found was 3.45 ml per liter whereas at corresponding depths in the Atlantic the quantity of oxygen is everywhere 5 ml per liter or more.

The calcium carbonate equilibrium in sea-water--This work is being carried out in collaboration between Moberg and Gee. They are studying the solubility of $CaCO_3$ in sea-water under various conditions of temperature, pH, and CO_2-content, with a view to determining the conditions under which it may be precipitated or dissolved in the sea. The results so far indicate that there are certain chemical factors involved which make the accepted theories of $CaCO_3$-solubility inapplicable.

Biology of Marine Organisms

Statistical methods--McEwen has published a paper "Methods of estimating the significance of differences in or probabilities of fluctuations due to random sampling" which resulted from efforts to develop and make available statistical methods for estimating the significance of differences in the abundance of marine plankton indicated by net-hauls made under different environmental conditions. It is a supplementary handbook prepared for the worker who is familiar with elementary statistical methods, but has to deal with special problems that involve small samples, high variability or skew-frequencies, and are thus outside the range covered by usual methods. Formulae, tables, and numerical problems to illustrate their use are presented to facilitate practical applications.

Another study by McEwen is that of plankton-swarming. This is a statistical study of the relation between plankton-abundance and haul-frequency or percentage of hauls containing more than a given number of organisms and was based upon a few of our records of both zooplankton and microplankton. The average values of observed abundance were found to exceed the theoretical value to be expected on the basis of probabilities assuming a uniform distribution of the organisms. The only explanation appears to be that the organisms occur in groups or swarms. Biologists working in this field had been led independently to a similar conclusion by general impressions.

McEwen has also been working on plankton-data regarded as time series. Preliminary studies of monthly averages of microplankton-counts and of corresponding physical data have been made by applying principles used in business statistics to determine the trend, typical seasonal changes, and cyclical variations. The usual methods of multiple correlation were applied to the cyclical variations or departures from the typical seasonal changes corrected for trend, a procedure justified since the time-factor is thus eliminated. The method appears to be suitable for analyzing a large mass of data corresponding to long time intervals.

Bacteriology--A. H. Gee has charge of the investigations on this subject at the Institution. According to him special mechanical equipment for the collecting and culturing of bacteria from the sea has been devised and perfected and found satisfactory on board ship. One of the devices introduced for handling bacteria in solid mediums on board ship has general application in bacteriological laboratories for some purposes for which the Petri dish has been used.

Direct cultural enumerations made at sea indicate that the water-populations in the vicinity of La Jolla are of the order of hundreds only per cc, and these are confined to the topmost 25 meters. This is an even sparser count than has been reported for samples of ocean-water elsewhere. Examination of the bottom-mud by the same method indicates, however, that there is appreciable activity on the part of readily recognized and cultured organisms. Molds, yeasts, and Actinomyces form an important part of the flora in addition to the bacteria proper. The attempt has been made to evaluate these enumerations of cultivable organisms. Direct microscopic counts of live, dead, and dormant organisms following different processes of concentration have been partially successful, and they indicate that the counts cited here and similar attempts elsewhere which are based on cultural methods, reveal only a small fraction of the bacteria actually present. Methods have yet to be devised for cultivating the organisms which are missed by conventional and special media hitherto recommended.

Phytoplankton--The work on phytoplankton has comprised the publication of papers by W. E. Allen giving the more obvious results of daily statistical records accumulated at inshore stations for five years, and a general statement of the work at such stations over a period of ten years. A report on winter collections of diatoms from surface-waters of the Gulf of Alaska and the Alexander Archipelago has been finished. Considerable progress has been made in microscopic examination of standard series of catches of phytoplankton in the Southern California region. A beginning has been made on a limited taxonomic study of diatoms of Java seas, and advance has been made in the study of kinds and degrees of variation of Pacific plankton-diatoms. Miss Easter E. Cupp is assisting Professor Allen in these investigations.

A brief study has been made of the relative dependability of filtration and concentration methods of collecting certain types of microplankton and a report published. An account of a design for a new kind of quantitative plankton closing net has also been published.

Plans have been formed for making experimental studies of diatoms. Plans for statistical treatment of the records for nearly 20,000 catches of phytoplankton taken by filtration in the last ten years have been made for the purpose of ascertaining what correlation there may be between the occurrence of the organisms and the physical and chemical features of the water from which they were taken. Most of the details of operation of these plans have been tested so that the statistical analysis can go forward as soon as the count records are completed. The statistical methods for this work were devised by McEwen and have already been mentioned in this paper.

Foraminifera--R. D. Norton has recently published (Tech. Bull. Scripps Inst., v. 2, 331-338) a paper entitled "Ecologic relations of some foraminifera." This is a study of the depth and temperature relations of about 550 species of foraminifera from the beach-line to a depth of 2,849 fathoms in the southwestern part of the North Atlantic, the Mexican Gulf, and Caribbean regions. E. M. Thorp has made estimates of the numbers of individuals of different species of foraminifera in a series of deep-sea samples collected between New London, Connecticut, and the Panama Canal Zone. G. Leslie Whipple is engaged on a study of the foraminifera associated with the marine bottom-deposits between Point Conception, California, and Panama. The three sets of studies by Norton, Whipple, and Thorp, supplement each other and are of value to geologists in that they will aid in improving the basis for inferring the physico-chemical conditions under which sedimentary rocks were deposited.

G. L. Whipple has prepared reports on larger foraminifera from the Miocene of Vitilevu, the Eocene of Eua, and the Quaternary of Vavau, to aid J. F. Hoffmeister and H. S. Ladd in their studies of the conditions under which the coral reefs of those islands have been formed. T. W. Vaughan is continuing his studies of the larger formaminfera, fossil and recent, of tropical and subtropical America, and has published twenty papers on the subject during the past few years.

Fishes--P. S. Barnhart for a number of years has made observations on the breeding seasons and spawning habits of local fishes. The species include many elasmobranchs (sharks and rays) and, among the Teleosts, the pipe-fishes, silversides, barracuda, mackerel, bonito, surf-fishes, rockfishes, cottids, and blennies.

The food of the California sardine (Sardinea coerulea) has been investigated by R. C. Lewis and the results published. The stomach contents of sam-

ples of fish taken throughout the year showed close correlation with the plankton in the water from which the fish were taken. The fish eat both phytoplankton and zooplankton, utilizing as food either both or whichever may be in the water. [A.E. Parr suggests (Ecology, v. 11, 465-468) the fish may not digest diatoms found in its stomach--they may be incidental only to catching zooplankton.] A. B. Keys has conducted investigations of the gas-exchange of fishes and the acclimation of a marine fish to fresh-water. A method, developed in the past year and a half, for measuring the gas-exchange of fishes was subjected to many tests with the result that it is now possible to use a standard technic in a wide variety of studies of the gas-exchange of aquatic animals in which a strictly physiological method for obtaining standard metabolism has been sorely needed. The paper describing the method in detail is now in manuscript and will shortly be submitted for publication. Results obtained by this method were utilized in a paper on the relation of the oxygen-consumption of fishes to the oxygen-tension of the water. It was shown that minor variations in the oxygen-tension of the water are without effect on the magnitude of the oxygen-consumption of fishes. The relation between the age of a fish and the oxygen-consumption per unit of weight was studied. It was found that the magnitude of the respiratory exchange varies inversely with the age of the fish, the younger animals having the higher rate of metabolism.

The physiological processes involved in the acclimation of the marine fish, Fundulus parvipinnis, to fresh-water were studied and the results embodied in a large paper now in press. The general problem of differences in the resistance of individuals to specific adverse situations was studied experimentally for a number of experimental conditions, and the results of the very numerous experiments analyzed statistically.

The effects of amytal anesthesia on fishes was investigated by A. B. Keys and N. A. Wells. It was found possible to induce prolonged anesthesia in fishes by means of amytal (iso-amylethyl barbituric acid) and the effects of such treatment on the respiratory processes were studied. These results are to be presented in a paper now in the course of preparation.

F. B. Sumner and A. B. Keys sought to analyze the optical stimuli responsible for changes of shade which occur in flatfishes, in response to changes in the nature of the background. It was shown, with fair conclusiveness, that the shade of the substratum (background) operates in connection with light from other parts of the visual field, and that the effective stimulus is the ratio of the light reflected from the background to the apparent source of illumination.

F. B. Sumner and Nelson A. Wells have for many months been conducting experiments with the small tropical fish, Lebistes reticulatus, with a view to ascertaining (1) the effects of the differences of temperature upon the number of vertebrae and other metameric structures and (2) the effects of differences in the shade and color of the surrounding containers upon the shade assumed by the fish--particularly upon the formation of pigment.

Besides those above listed, the results of other researches on the ecology and physiology of marine organisms are worth mentioning. One of these is a study by Prof. W. R. Coe of Yale University of the organisms that attached themselves to blocks that were suspended from the Institution's pier and allowed to remain suspended during definite times.

Geologic processes--E. M. Thorp has nearly ready for press a paper on marine bottom-deposits, already mentioned, and G. L. Whipple is studying the

bottom-deposits in the eastern Pacific between Santa Barbara, California, and Panama. Other investigations on the large number of samples at the Scripps Institution are contemplated.

T. W. Vaughan is making a quantitative investigation of the rates of cliff-recession in the vicinity of the Scripps Institution and he is trying to correlate the rates with as many factors as practicable, such as toughness and structure of material and height of cliff. Other factors, such as momentum of waves, shore and near-shore currents, and slope of bottom at and near the beaches, are being considered, but the phenomena are very complex and difficult to analyze. G. L. Whipple is assisting with the field and laboratory work. G. F. McEwen is helping with the study of water-movements and the intricate mechanics. Because of its bearing on this problem, a detailed chart, scale 1 to 10,000, of the sea-bottom off the sea-front of the Scripps Institution's property, made in cooperation between the Coast and Geodetic Survey and the Scripps Institution, is mentioned.

From foregoing statements it is obvious that to understand the geologic processes operative in the sea knowledge of all branches of oceanography, physical oceanography, the chemistry of sea-water, and marine biology, is necessary and fundamental.

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SOME PRELIMINARY RESULTS OF THE COAST GUARD'S MARION EXPEDITION TO DAVIS STRAIT

Edward H. Smith

Although it has been about two years since the Coast Guard Cutter Marion made her oceanographical survey of Davis Strait and lower Baffin Bay, the scientific report of the Expedition has not yet appeared in print. The section on oceanography and ice is being prepared by Lieutenant Commander Edward H. Smith, the leader of the Expedition. Unfortunately his whole time has not been available for the Marion Expedition report due to other pressing duties in the Coast Guard, but, nevertheless, it is hoped that the report will soon be completed and ready for distribution. In the meantime, one short notice has appeared (Science, v. 68, 469-470, 1928), giving a brief report of the area covered and the nature and number of the observations and a few of the major discoveries made. Since the appearance of this note, the oceanographical data have been subjected to Bjerknes' dynamic formulae and several interesting maps of the surface topography of Davis Strait have been constructed. We wish to describe some of the outstanding features of the circulation, which have been revealed for the first time, as they prevailed during the summer of 1928 between Greenland and North America.

First we wish to call attention to Figure 1 showing the general bathymetrical features of Davis Strait as determined by the 2,500 soundings of the Marion's fathometer, and as further supported by previous authentic wire-soundings. The number of deep soundings existing prior to the work of the Marion was very scanty and areas as extensive as 30,000 to 40,000 square miles contained not a single depth-record. The coastal shelf of west Greenland on Figure 1, it will be noticed, is much narrower and steeper than that shown on present-day maps, and the Labrador shelf is somewhat wider. It is also relieved by a depression about 40 miles out from the coast which extends as a

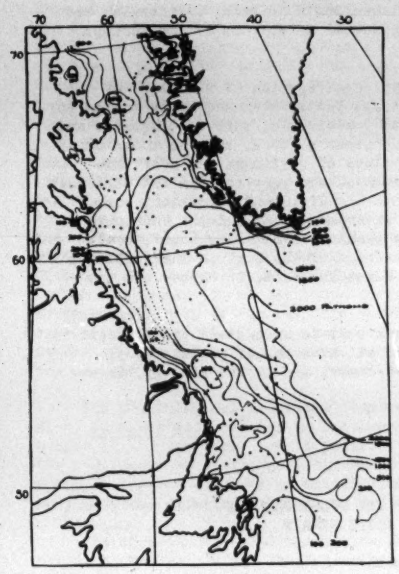

Fig. 1--Bathymetrical features of Davis Strait from soundings of Marion Expedition

trough along the entire front from Cape Chidley southward to Newfoundland. The depth-survey corroborates previous maps, namely, that the broader, shallower shelf is located on the American side of Davis Strait, and the currents, largely in consequence, are much wider and slower moving on the western side of Davis Strait than they are on the eastern side.

The dynamic topographical map of the sea-surface of Davis Strait, Figure 2, has been constructed in accordance with the methods described in Coast Guard Bulletin No. 14. In regions near the coast where depths are less than 1,500 decibars, we have followed the methods used by Jacobsen and Jensen in their paper "Examination of hydrographical measurements from the research vessels Explorer and Dana in the summer of 1924."

This is the first time that the currents over this important ice-region have ever been charted--a task that could not have been accomplished without the raw data of the sort collected by the Marion. The results of the Marion Expedition not only reveal for the first time the hydrographical circulation between Greenland and North America, but allow us to trace by logical scientific methods the movement of the ice over a major part of its northern pathway.

The lowest point in the sea-surface of Davis Strait as shown on the dynamic topographic map, Figure 2, was found at the Marion station 947, located about 130 miles seaward of Hamilton Inlet, Labrador, and in the deep water just off the continental slope. This point was called zero and used as a bench-mark above which the surface rose on all sides. This dynamic height from a point in the sea at station 947, where the pressure was 1,500 decibars, to the sea-surface was 1,454.46 dynamic meters above the 1,500-decibar plane. In this manner the particular dynamic height of the sea-surface was calculated for each of the 191 stations which the Marion took, and equal heights were connected by lines called isobaths. The dynamic isobaths in Figure 2 have been drawn for every two-dynamic-centimeter change in altitude. By obtaining these values for so many stations scattered over the Davis Strait area, it has been possible to construct a horizontal projection for Davis Strait as a whole, from Disko Bay, Greenland, to the latitude of Cape Race, Newfoundland.

In the interpretation of the motion indicated by the dynamic gradients

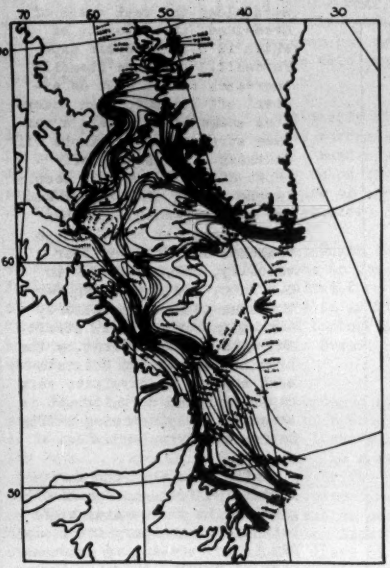

Fig. 2--Dynamic topographical map of sea-surface of Davis Strait, Marion Expedition

drawn on Figure 2, we should bear in mind that results are comparative and not absolute. For example, it has been assumed that the isobaric surface of 1,500 decibars, chosen as a base of reference, approached more nearly to a level position than any other isobaric surface, and, furthermore, that the sea-surface embraced the greatest variation in dynamic potential as we proceeded from place to place. The facts (a) that the rate of free motion varies directly with the obliquity of isobaric surfaces and (b) that the greatest intensity of circulation is seated in the upper levels of the sea, justify as sound our procedure in assuring the 1,500-decibar plane was nearest level and that the sea-surface was greatly inclined.

The dynamic topographic map, Figure 2, resembles the ordinary isobaric weather-map in appearance and the circulation of the water resulting from the topography is essentially similar. The water particles in the region where the surface stands dynamically highest tend to flow into those where it stands dynamically lowest, but the effect of Earth rotation deflects the actual currents to stream-lines for all practical purposes parallel to the isobaths. All motion is taken account of in calculations except that due to the tides and to the friction of the winds in the surface-layers. But both these factors, due to the geographical configurations and to the season of the year, exert only a minor effect on the area in question.

The steepest gradients are found, as shown on Figure 2, wherever the dynamic isobaths become tightly packed. They are (a) off the west coast of Greenland from Ivigtut northward to Holsteinborg, (b) on the northern side of Hudson Strait, and (c) on the steep part of the Labrador shelf opposite Nain. In the case of (a) the gradient measured 24 dynamic centimeters in a distance of 20 miles, at (b) 8 dynamic centimeters in 12 miles, and at (c) 22 centimeters in 12 miles. Like the underlying bottom-features, the dynamic gradient, as shown on Figure 2, is narrowest and steepest on the Greenland side of Davis Strait and widest and most gentle on the American side.

A general survey of the oceanic circulation is presented on Figure 3, which is based on the form and direction of the dynamic isobaths just shown on Figure 2. A fairly swift ocean-current as a warm, salty indraught sets north-

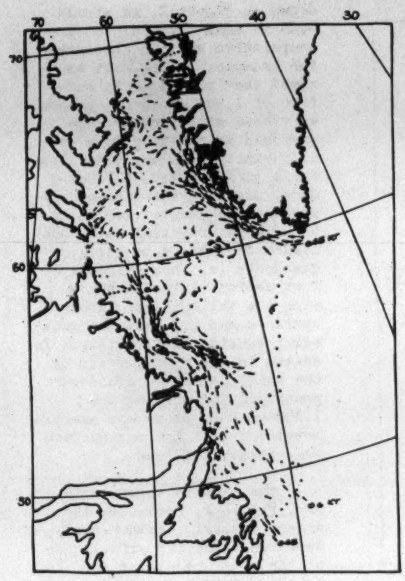

Fig. 3--General survey oceanic circulation, Davis Strait, Marion Expedition

ward along the west coast of Greenland, the velocity of which is 0.45 knot off Cape Farewell. Its rate steadily increases to 0.8 knot on arrival off Ivigtut, then moderates somewhat to attain a maximum strength of 1.1 knots off Godthaab. A westerly offshoot of small quantity occurs in the offing off Ivigtut and two more off-shoots farther north along its path. These branches are of much lower velocity, being only 0.2 to 0.3 knot, and move along very tortuous paths in a westerly direction across Davis Strait. The warm, salty current by the time it has reached Holsteinborg appears to dissipate very rapidly and no definite set is shown actually entering Baffin Bay on the Greenland side.

The fact that the Marion Expedition found no dynamic gradient in the coastal water between Holsteinborg and Disko Bay is not conclusive evidence that no current prevails there or that it does not penetrate into Baffin Bay at times. Since the dynamic calculation reveals no absolute values but furnishes comparative results only, the question is open to one other possibility, namely, that on August 1-7, 1928, the entire column of water was moving northward, surface and bottom, at the same rate. This is a case in which no dynamic tendency would be indicated by the raw data. The fact, however, that the Greenland shelf broadens very noticeably in this locality probably tends to turn off the gradient current and leads us to believe that at the particular time when the Marion passed through this region, the current did not penetrate much farther north than Holsteinborg. What actually happens at this place is probably an interrupted flow by which water-masses from the Atlantic may be carried into Baffin Bay during the periods when the west Greenland Current is at its flood.

The cold current from the north is shown following a course out of Baffin Bay on the American side and thence down the Labrador coast from whence the famous stream takes its name. The velocities are somewhat lower than on the Greenland side, but the general width of the current is also much greater. We

find the water flowing at 0.3 to 0.4 knot off southern Baffin Land but increasing to a maximum of one knot off mid-Labrador. The current in the southern sector of the Marion's survey broadens out perceptibly, reflecting the increased width of the continental shelf in this region, and simultaneously the movement decreases to about 0.2 knot.

The Labrador Current is noticeably affected off Hudson Strait and Belle Isle by the coursing of the tidal currents through these openings. Another feature of the icy current is its tendency to divide longitudinally into parallel bands. For example, off Nain on the edge of the slope, we find the water setting southward at the rapid rate of one knot but farther inshore, about 15 or 20 miles from the coast, the velocity is only 0.5 knot.

The real object of the Coast Guard in sending the Marion Expedition northward during the summer of 1928 was to learn the general paths which the icebergs follow from the time they are calved from the glaciers until they appear south of Newfoundland. The Coast Guard is seeking all information possible in order to solve the outstanding problem facing the International Ice Patrol. A dynamic topographical map of the Davis Strait region affords the best answer to the question of ice-movements.

There is no doubt of the soundness of employing a dynamic topographical map based on Bjerknes' formulae of hydrodynamics to trace the prevailing drift of deep-floating objects such as icebergs. On several different occasions on Ice Patrol, especially during the ice seasons of 1926 and 1927, I have had the opportunity to compare the actual drifts of many icebergs for hundreds of miles south of Newfoundland with the stream-lines of the gradient currents as shown on the dynamic topographic maps and in nearly all cases the agreement is good. Since the current-map of the Marion Expedition is similar in every respect to those constructed on Ice Patrol, there is every reason to believe the similarity between calculated currents and ice-drifts holds equally true for Davis Strait.

The icebergs, once adrift in Davis Strait, although affected by the winds, are mainly controlled by the prevailing oceanic circulation. The current-survey of the Marion therefore allows us for the first time to definitely settle many questions which have been asked regarding the drift of arctic ice. For instance, there has been much discussion in various arctic publications from time to time whether any of the pack-ice and icebergs which drift southward to Cape Farewell, Greenland, continue either toward Newfoundland or westward to join the ice carried in the Labrador Current. If we refer to the Marion's current-map, Figure 3, we will see that the bergs less than 30 miles off Cape Farewell will be carried northwestwards, parallel to the coast, at a rate of 11 miles per day. Their rate of travel constantly increases until off Cape Desolation they are being borne along at 19 miles per day. Bergs on the outer edge of the current would, of course, not move so rapidly as those nearer its axis. Off Cape Desolation we will observe that the ice receives its first opportunity to leave the coastal belt, developing into a broad and slow tortuous drift. The icebergs which are so deflected will move slowly to the westward at a rate of four to six miles per day, and it will take four months for such ice to cross Davis Strait opposite Ivigtut and two months opposite Godthaab. If the first of the season's bergs, which arrive at Cape Farewell in April, were to follow in some of the off-shoots of the West Greenland Current there is a possibility that they would reach Labrador about the middle of August, but the likelihood of such an event is extremely small. Also the fact that the most opportune time is summer when the central water-masses of Davis Strait are warmest, further precludes the likelihood of such a journey.

Another interesting point which has been brought out by the Marion Expedition is the unreliability of ocean-currents as a means of berg-transportation. For example, the dynamic maps clearly show the current to be highly belted and banded, and this causes the bergs to be much assorted. If a berg be caught and held in a narrow, swift band of current it is assured a rapid transportation towards the south while another berg within plain view may remain for some time in the north, floating in dead water. Such variations in the circulation over a short distance cause wide variations in the drift of bergs and two bodies of ice detached from the same glacier on the same day may be separated by as much as two or more years in their Atlantic arrival.

The Marion Expedition discovered that the bergs which remain out near the continental edge off the Labrador coast drift the fastest but even in this narrow track it is observed they will not be transported at a constant rate. A comparison of the Marion data, for instance, with that of the Chance indicates that the Labrador Current may vary in velocity from 9 to 25 miles per day. We have observed similar wide fluctuations in the Labrador Current farther south around Newfoundland and also noted the fact that such non-periodical pulsations may be initiated in a very short space of time.

The undulating course of the stream-lines belonging to the Labrador Current along the coast is a feature which is clearly shown on Figure 3. Note the well-marked inshore swirl to the current which is shown about half-way down the Labrador coast between Cape Chidley and the Strait of Belle Isle, and which is probably caused by a major embayment located in the coastal shelf in this vicinity. The stream, deflecting as it does to the right of a straight-away course, has a strong influence on the drift of the icebergs and normally carries large numbers in towards the coast at this point. Such a behavior of the ice is corroborated by the distribution of stranded bergs recorded along the Labrador coast at various times and especially during the summer of 1928, when several hundred were observed set on the shore north of Hamilton Inlet.

Little work has yet been done on analyzing the material from the viewpoint of the distribution of salinity and temperature as contained in some 2,000 odd observations made by the Marion. We have, however, proceeded far enough to state that there is no indication whatsoever of a branch of the Atlantic Current which has often been represented as diving beneath the surface south of Greenland and penetrating through the mid-depths of Davis Strait, finally to emerge in northern Baffin Bay as a warm oasis, commonly known as "North Water." The distribution of temperature (as well as salinity) in the east to west vertical cross-section of Davis Strait are those quite typical of boreal charactered water for the summer period. There was a surface-layer of sun-warmed water below which filling the mid-depths lay cooler masses insulated from the change of seasons and saltier than that mixed by melting ice and precipitation. In the lower depths lay water of 34.90 o/oo and a temperature 2.°8 to 2.°6. The only portion of the sections which show evidence of Atlantic importations is in the surface-layers adjacent to the Greenland coast where the saltiest and warmest water extended as a tongue from Cape Farewell to Cape Desolation with a salinity of 35.00 o/oo and a temperature of 6.°0 C.

The presence of bottom-water of 2.°6 C and 34.90 to 34.87 o/oo distributed quite generally in the deeper parts of the Greenland-North American trough, raises the much-discussed question of its origin. The fact that the water is separated from the surface seasonal zone of some 200 meters thickness by hundreds of meters of water maintaining year after year a constant salinity and temperature fresher and warmer, apparently precludes for the bottom-water a surface-derivation. This statement applies to the stability conditions for

the entire Davis Strait area but of course leaves as an open source other northern regions of the Atlantic basin as possible regions of production. For example, the bottom-water of Davis Strait may come from the eastern side of Cape Farewell where salty water is severely chilled in winter and heavy sinking masses thus develop there. Of course, there is always the probability that such bottom-water even as remote as the head of Davis Strait may be part of a slow creep which is traceable southward into the antarctic. This question needs further thought and study.

The Marion found that the water and ice which moves around Greenland is markedly separated from the currents and ice in the American regions. A northward indraught along west Greenland and southward outflow along eastern North America set up a more or less anti-clockwise gyration of Davis Strait waters. The striking absence of currents and icebergs outside of coastal shelves has been strongly emphasized. Four areas, relatively a small part of the North Atlantic Ocean, have already been studied according to Bjerknes' methods of hydrodynamics and the results published. Helland-Hansen and Nansen in "The eastern North Atlantic" have covered the waters off the European coast as far west as the 30th meridian; Jacobsen and Jensen have likewise surveyed the ocean currents in the Faroe-Shetland Channel; Wüst, in a publication "Florida and Antillenstrom" mapped the circulation of the Strait of Florida; Bigelow has discussed the hydrodynamics of the Gulf of Maine, south to Martha's Vineyard; and the International Ice Patrol, 1926 and 1927, has published several dynamic surveys of the waters around the Grand Bank. We are gradually making progress as more and more oceanic masses are being thus surveyed dynamically. It is hoped that the Marion Expedition report will soon give similar information for the interesting waters between Greenland and North America.

U. S. Coast Guard,
Washington, D. C.

THE SEVENTH CRUISE OF THE CARNEGIE

J. A. Fleming

On the afternoon of November 29, 1929, there ended tragically the last of the seven world-cruises of the non-magnetic ship Carnegie of the Carnegie Institution of Washington. During the three cruises in the Pacific Ocean of the brigantine Galilee chartered by the Institution from 1905 to 1908 and during the first six cruises of the Carnegie in all oceans from 1909 to 1921, chief emphasis had been placed upon the magnetic and electric programs to develop the distribution-surveys in terrestrial magnetism and electricity over the oceans, with only a moderate amount of oceanographic and meteorological observations. The seventh cruise of the Carnegie was planned to determine magnetic secular-variation, to extend the atmospheric-electric survey, and to undertake comprehensive work in physical, chemical, and biological oceanography following a program developed and already described by Fleming and Ault (Program of scientific work on Cruise VII of the Carnegie 1928-31, Zs. Ges. Erdk., Ergänzungsheft v. 3, 41-55, 1928). Nearly one-half of the cruise as planned had been completed upon the arrival of the Carnegie at Apia, November 28, 1929, and the extensive scientific program of observation in terrestrial magnetism, terrestrial electricity, chemical oceanography, physical oceanography, marine biology, and marine meteorology had been realized practically in every detail. The development of practical technique and instrumental appliances for oceanographic work aboard a sailing vessel had been outstandingly successful.

It was shortly after one o'clock November 29, while in harbor at Apia, Western Samoa, and filling the gasoline storage-tanks, that an explosion took place resulting in the deaths of Captain Ault and Cabin-Boy Anthony Kolar, injuries to the engineer, the mechanic, and three seamen, and the total destruction of the ship with all her equipment. The explosion apparently was simultaneous with the throwing of the switch by a mechanic to connect the winch on deck with the dynamo; it may be that an explosive mixture of gasoline, possibly accumulated in the engine-room during the lunch-hour interruption, was ignited by a spark. The closing up of the affairs of the vessel at Apia occupied two months, this work being in charge of W. C. Parkinson, senior scientific officer and second in command.

Thanks to the policy of the Department since its formation and to the characteristic promptness of Captain Ault, all records, reports, and samples obtained to arrival at Pago Pago, November 18, 1929, had been mailed or forwarded by freight from that place and have now been received in Washington for discussion and examination. The experience and acquired technique of the scientific personnel of the Carnegie during the cruise will add much to the effective preparation of the scientific results and data. Already preliminary manuscripts have been prepared and material for a narrative account of the cruise has been assembled. Among the reports in preparation of special oceanographical interest are those on the physical oceanography of the routes traversed, on submarine configuration and oceanic circulation, on the determination of salinity at sea, on gravity-observations, on the program of chemical and biological results, on the meteorological observations, on the results in terrestrial magnetism and atmospheric electricity, on the accuracy of depth temperature-determinations, on methods of collecting bottom-samples, and on an instrument for determining hydrogen-ion concentration at sea without the use of buffer solutions.

Table 1 summarizes the 16 legs of the cruise which were completed before the disaster and also indicates the number of magnetic stations occupied. The preliminary results of the magnetic observations have already appeared in print, and these data have been embodied in the new edition of the United States Hydrographic Office magnetic world-charts for the epoch 1930.

The extent of the oceanographic work may be visualized from Table 2. During the cruise there were 162 series-stations, thus at an average distance apart of less than 300 nautical miles. Figure 1 delineates the course followed by the vessel from May 1928 to November 1930 and also that portion of the cruise as planned which could not be completed.

Several different types of bottom-samplers were used, among these being the Meteor tube, the turtle snapper-type, and the Sigsbee tube. The snapper-type proved most successful in the form as modified by James Ross of the Scripps Institution of Oceanography at La Jolla, California. Dr. Vaughan, Director of that Institution, was kind enough to have several of these improved snappers made for the Carnegie. The chief advantage of this type is that it will bring up a specimen of almost any kind of bottom-material--ooze, mud, clay, sand, or even fragments of rock. The action of this bottom-sampler was greatly improved as a result of the experience and developments on the Carnegie. Through countersinking of the lead weight, thus permitting it to fit down over the closing spring immediately above the jaws, the operation was much improved. The 60-pound lead weight was in two halves, each being suspended by a wire from the hook on a releasing device installed at the end of the shaft of the snapper, the bottoms of the two halves being fastened together by two staples driven in fairly tight. On striking the bottom the hook released the wires, allowing the

Table 1--Summary of ship's log showing legs of Carnegie's Cruise VII and magnetic stations

Leg	Abstract of ship's log						Magnetic stations		
	Departing	From	Arriving	At	Distance	Days at sea	D	H	I
	1928-9		1928-9		miles				
1	May 11	Newport News	Jun 9	Plymouth	3,669	29.3	29	12	11
2	Jun 18	Plymouth	Jun 22	Hamburg	614	4.1	2	2	2
3	Jul 7	Hamburg	Jul 20	Reykjavik	1,329	13.0	11	6	6
4	Jul 27	Reykjavik	Sep 16	Barbados	5,715	51.8	76	25	25
5	Oct 1	Barbados	Oct 11	Balboa	1,361	9.7	15	5	5
6	Oct 25	Balboa	Dec 6	Easter Island	4,788	41.9	58	19	19
7	Dec 12	Easter Island	Jan 14	Callao	3,334	32.9	38	15	15
8	Feb 5	Callao	Mar 13	Papeete	4,470	35.8	63	17	17
9	Mar 20	Papeete	Apr 1	Pago Pago	1,274	12.2	20	6	6
10	Apr 5	Pago Pago	Apr 6	Apia	80	0.8	..	..	..
11	Apr 20	Apia	May 20	Guam	3,914	28.8	49	13	13
12	May 25	Guam	Jun 7	Yokohama	1,447	13.2	20	6	6
13	Jun 24	Yokohama	Jul 28	San Francisco	4,839	34.9	40	17	17
14	Sep 3	San Francisco	Sep 23	Honolulu	2,186	20.1	27	9	9
15	Oct 2	Honolulu	Nov 18	Pago Pago	5,777	47.2	72	23	23
16	Nov 27	Pago Pago	Nov 28	Apia	80	0.8	..	..	..
Totals					44,877	376.5	520	175	174

Table 2--Oceanographic stations on Carnegie's Cruise VII

Leg	Series[a]	Phys. and chem.	Biological[b]				Sonic depths	Bottom samples
			Net	Pump	Surface	Samples		
1	6	6[c]	8	..	7[c]	49	58	1
2	..	..[d]	..	..	..[d]	..	..	..
3	2	2[d]	2	..	4[d]	10	40	1
4	22	22	22	21	7	129	205	6
5	4	4	4	3	..	19	29	1
6	19	19	20	18	14	116	71	10
7	17	17	17	16	23	118	73	13
8	17	17	17	17	2	88	210	15
9	6	6	6	5	1	26	64	..
10	..	..	..	..	..	..	6	..
11	14	14	14	14	1	86	159	3
12	5	5	5	5	..	32	50	5
13	17	17	19	16	2	103	166	7
14	10	10	12	10	24	75	96	9
15	23	23	23	23	23	163	272	16
16	..	..	..	..	..	..	1[e]	..
Totals	162	162	169	148	108	1014	1500	87

[a] Salinities and temperatures were obtained approximately at the following depths: 0, 5, 25, 50, 75, 100, 200, 300, 40 500, 700, 1,000, 1,500, etc., down to bottom or to maximum of 6,000 meters.

[b] Samples were obtained usually at 0, 50, and 100 meters.

[c] There were also 35 surface physical and chemical stations.

[d] There were also 38 surface physical and chemical stations.

[e] Record lost with the Carnegie.

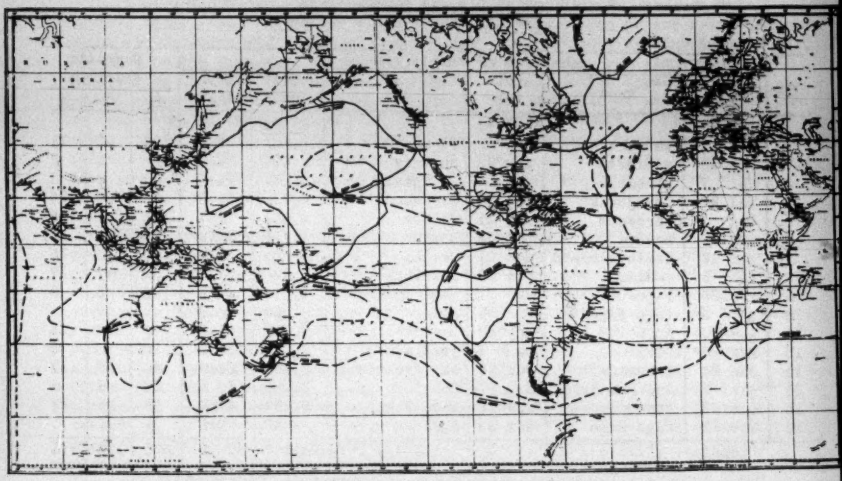

Fig. 1--Cruise VII of the Carnegie, May 1, 1928 to November 29, 1929 (Broken lines show portion not completed; this seventh cruise added 52,000 statute miles to the 291,000 covered during the first six cruises of the vessel between 1909 and 1921--a total of 343,000 miles in all oceans)

two weights to fall apart outward from the top, thus forcing the lower staples out and leaving the weights fall free, the snapper at the same time being driven into the bottom with such force by the 60-pound weight that the catches were invariably released and the snapper always came up full and closed. The distribution in the southeastern Pacific of the various kinds of bottom-material as determined from examinations of 38 samples of the Carnegie and those of the Agassiz and Challenger expeditions taken in that region has been studied by Seiwell and is shown in Figure 2. In addition to the deep-sea deposits found at relatively short distances off shore in the vicinity of the islands, there are small and more or less isolated patches of volcanic materials--the products of submarine eruptions as found at several of the Carnegie stations.

Details regarding the sonic depth determinations and their control through determinations made by pressure thermometers and corrected wire-depths, the character of the temperature-determinations and their accuracy, and a description of the salinity-apparatus with some account of the results are being given in other papers presented before the Section.

The chemical program other than for the determination of salinity was confined largely to a study of the conditions affecting the distribution of organisms at sea. Thus salt-water samples were analyzed for hydrogen-ion concentration, phosphates, silicates, and dissolved oxygen. In commenting on the results in the Pacific, H. W. Graham, biologist and chemist, says that these "show that the waters may be divided into three general layers: An upper layer where an active plant and animal life is maintained, a middle layer in which a decom-

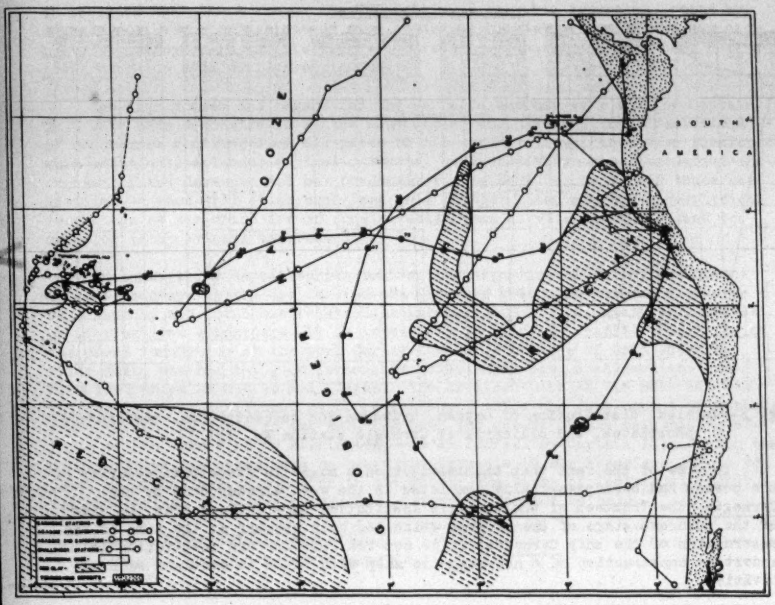

Fig. 2--Distribution of bottom-material in the southeastern Pacific

position of organic remains is taking place, and a lower layer which represents water that has been conducted from polar regions. The upper layer shows high values of dissolved oxygen but low values of phosphates, silicates, and hydrogen-ion concentration. The middle layer is low in oxygen but high in phosphates and silicates, and is relatively less alkaline. The lower layer tends towards conditions at the surface with again higher values of dissolved oxygen and lower of phosphates, silicates, and hydrogen-ion concentration." Figure 3 gives typical graphs of the chemical analyses of samples obtained at various depths.

The meteorological program, in addition to ordinary observations, included determinations for studies of several special problems in atmospheric conditions over the oceans. Temperature and humidity lapse-rates from quarter-deck to masthead were recorded automatically by an electric resistance multi-thermograph with wet- and dry-bulb elements at various heights. Continuous records of sea-surface temperatures were obtained by a bulb-and-capillary recorder. Continuous records were also made of humidity using a recording aspiration-psychrometer, of air-temperature, and of atmospheric pressure. A feature of the meteorological program which was being carried out most successfully was that of the use of pilot-balloons to determine the direction and velocity of air-currents to great heights--171 such flights were observed. Thirty-eight measurements of the rate of evaporation were made when conditions were favorable.

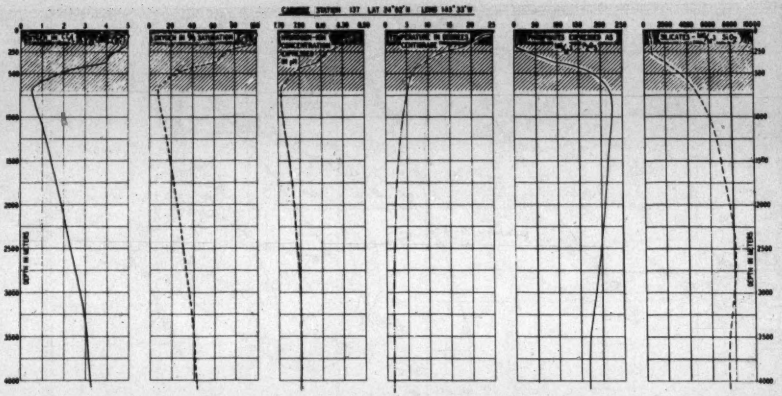

Fig. 3--Vertical distribution of oxygen, hydrogen-ion concentration, temperature, phosphates, and silicates at Carnegie station No. 137

In view of the fact that the Institution's magnetic distribution-survey of the oceans had been practically completed in the work already done by the Carnegie, the Trustees of the Carnegie Institution have decided that, in view of the advanced stage of the program which had been reached at the time of the destruction of the ship Carnegie, it is not feasible for the Institution to undertake construction of a non-magnetic ship with which to continue such activities.

It is hoped, however, that steps may be initiated promptly through cooperation of oceanographic interests in the United States for a ship or ships specially designed for oceanographical researches with occasional magnetic and electric observations in all oceans in continuation and development of the plans which were being so well executed on the Carnegie. It is in this way chiefly that the experience and technique acquired in many years of preparation, of development of instruments, and of cruising could be capitalized to benefit future oceanographic research--the need of which was being surely indicated by the Carnegie results and the importance of which to future scientific and economic advances in all geophysical sciences cannot be overstated. Such a vessel need not be non-magnetic in construction except perhaps near a location on board designated for determining the magnetic elements. Thus the practical difficulties of non-magnetic material and equipment which increase construction and maintenance costs would be limited while the greater power-requirements for the oceanographic program and its expansion could readily be provided using standard and, therefore, more rugged equipment.

The prompt and generous assistance of all persons and organizations who could be of help in the emergency of the Carnegie disaster is marked. The United States Navy has given the privilege of unlimited radio communication from Washington with the Naval Station at Pago Pago--without this privilege the difficulties would have been greatly increased. In Western Samoa and in American Samoa every help has been extended by the local authorities through the Administrator of Western Samoa and the Governor of American Samoa. It will

be recalled that there was on the Carnegie much valuable apparatus loaned by oceanographical organizations of many countries besides our own which was completely destroyed; in practically every case the organization concerned has taken over the loss of such equipment.

The high esteem and regard for the Carnegie and her work and for Captain Ault have been demonstrated by the many cablegrams, letters, and resolutions of condolence received from all parts of the world. These, too, have furnished much additional evidence of that universal good-will which has, throughout the cruises of the Carnegie and her predecessor, the Galilee, so marked these activities--a good-will which doubtless will further stimulate those geophysical researches at sea which are of considerable human service and which tend to make for international understanding.

In summarizing an enterprise such as the magnetic, electric, and oceanographic surveys by the Galilee and the Carnegie which has covered a quarter of a century and which has required cooperative effort and unselfish interest of many skilled scientists, it is impossible to allocate credit. Peters laid the broad foundation of the work during the early cruises of both vessels, while Ault, who had the good fortune to serve under him, continued and developed that which he had so well begun. The original plan of the work and its inspiration were envisioned by Director Bauer, while the development of methods and apparatus suitable to the work has been the result of the painstaking efforts of his coworkers at Washington. In an address by Captain Ault during the commemorative exercises held on board the Carnegie in San Francisco August 26, 1929, he emphasized this matter in the words "The story of individual endeavor and enterprise, of invention and accomplishment, cannot be told."

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SOME OCEANOGRAPHIC RESULTS OF THE CARNEGIE'S WORK IN THE PACIFIC--THE PERUVIAN CURRENT

H. U. Sverdrup

The surface-currents of the Pacific are fairly well known, and are represented schematically in Figure 1. It is seen that the surface circulation is approximately symmetrical on both sides of the thermal equator, which is found in latitude 6° to 8° north. The equatorial counter-current runs towards the east along the thermal equator and north and south of this line we find two enormous vortices. The southern part of the vortex on the southern hemisphere is formed by the west wind-drift, which when striking the coast of South America is deflected to the north following the coast as the conspicuous Peruvian Current.

The Peruvian Current carries cold water of low salinity (Fig. 2) along the coasts of Chile and Peru and is the most outstanding current on the southern hemisphere. The relation of this current to the surface-currents has been recognized a long time ago, but the relation of this current to the sub-surface circulation has not been established. However, it is generally assumed that the low salinity and low temperature of the surface-waters off the coast of Peru are due to upwelling of the deep water, but nothing has been known definitely as to the depth from which the water comes. The observations of the Carnegie show clearly that the possible vertical upward motion is a relatively small-scale

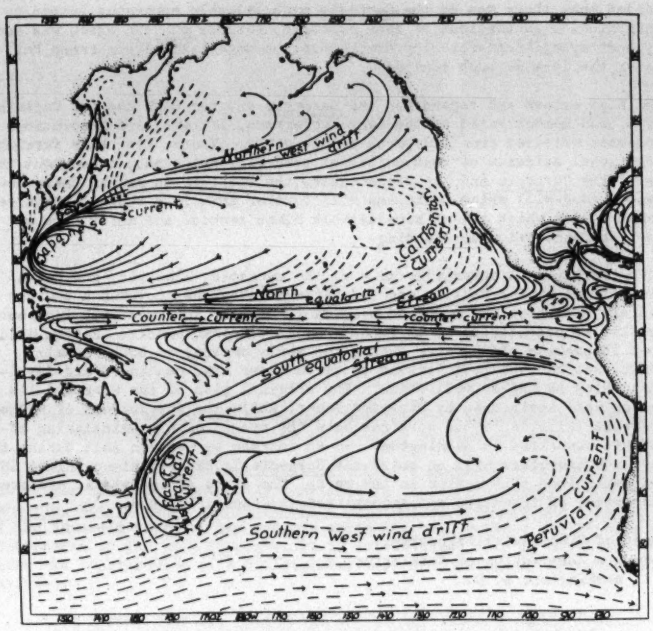

Fig. 1--Schematic representation of the surface-currents in the Pacific (after Johnstone)

phenomenon within the Peruvian Current and is limited to the upper 200 to 300 meters.

Before treating the Carnegie observations from the Peruvian Current it is necessary to draw attention to the general character of the sub-surface circulation in the Pacific which recently has been disclosed by the investigations of Mers, Wüst, and Defant, who principally used the salinity-observations of the Challenger Expedition of 1874-1875. Figure 3 shows the distribution of the salinity in a vertical section through the central part of the Pacific. It is seen that the distribution is fairly symmetrical around the thermal equator, indicating a symmetrical sub-surface circulation within both hemispheres. We are here not concerned with the currents near the surface, but shall fix attention at the conspicuous salinity-minimum which can be followed in both hemispheres from high latitudes towards the equator. In high latitudes the low-salinity values are found at the surface, but approaching the equator they are met at a depth of about 700 meters. The two tongues of water of low salinity indicate beyond doubt that water, which has been at the surface in the sub-arctic or sub-antarctic regions where the salinity has been reduced on account of excessive precipitation, is carried back to the equator at a depth of 600 to 1,000 meters. The salinity within this current increases when approaching

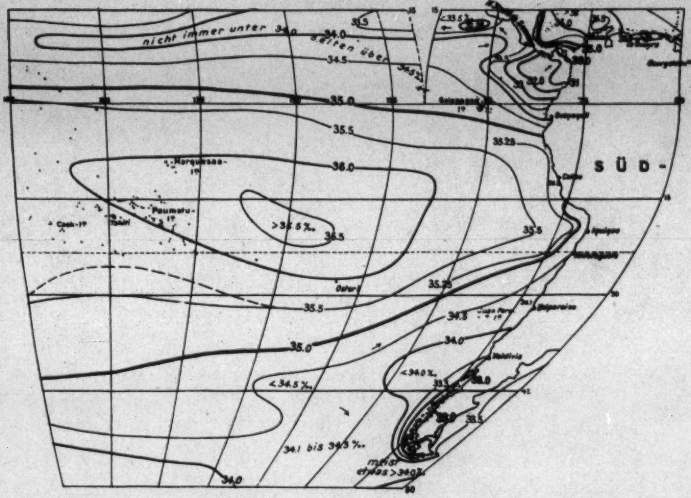

Fig. 2--Salinities of the surface-waters west of South America (after G. Schott)

the equator on account of admixture of saltier water from above.

It has not been possible to arrive at any results as to the character of the deep-sea currents in the Pacific because of lack of observations from the great depths. It is to be hoped that the observations from the Carnegie will give new information on this point, but this problem will not be approached now. We shall confine ourselves to a study of the relation between the Peruvian Current and the intermediate Antarctic Current which evidently is characteristic of the entire southern Pacific.

The Carnegie crossed the Peruvian Current several times as is evident from the chart in Figure 4 on which the oceanographic stations have been entered. Stations 40 to 80 are located within or in the neighborhood of the Peruvian Current. On the basis of the observations from these stations it is possible to construct five sections, showing the vertical distribution of the salinity in the same manner as shown in Figure 3. Three of these sections, from stations 40 to 45, 70 to 80, and 60 to 50, run approximately at right-angles to the Peruvian Current and may be termed cross-sections, while two, from 50 to 45 and from 60 to 70, are almost parallel to the Current and may be called longitudinal sections.

Let us first examine the cross-sections, beginning with the most northerly (Fig. 5). In this section we immediately recognize the intermediate layer with low salinity, representing the intermediate Antarctic Current. It is of interest to note that the axis of this body of water is horizontal.

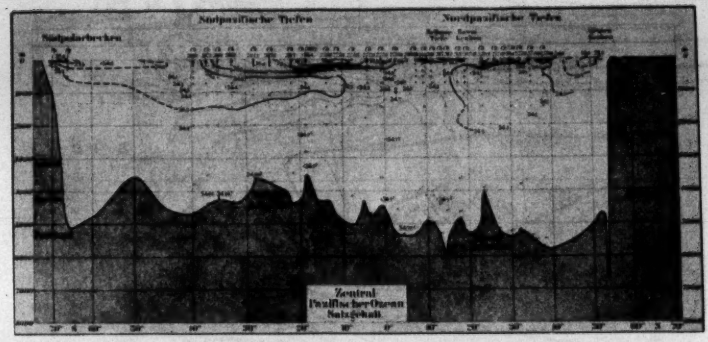

Fig. 3--Vertical section showing the distribution of the salinity in the central part of the Pacific (after G. Wüst)

Fig. 4--Distribution of the oceanographic stations of the Carnegie, 1928-29 (There was a total of 162 series-stations--128 in the Pacific and 34 in the Atlantic)

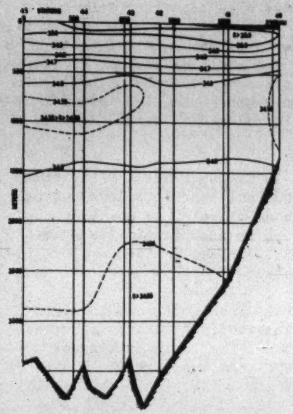

Fig. 5--Salinity-section based on Carnegie stations 40-45

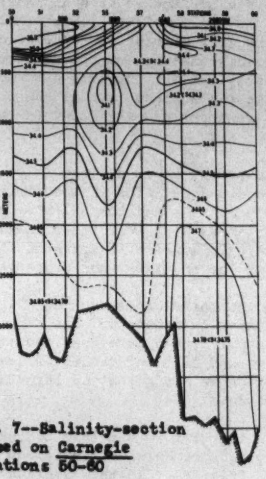

Fig. 7--Salinity-section based on Carnegie stations 50-60

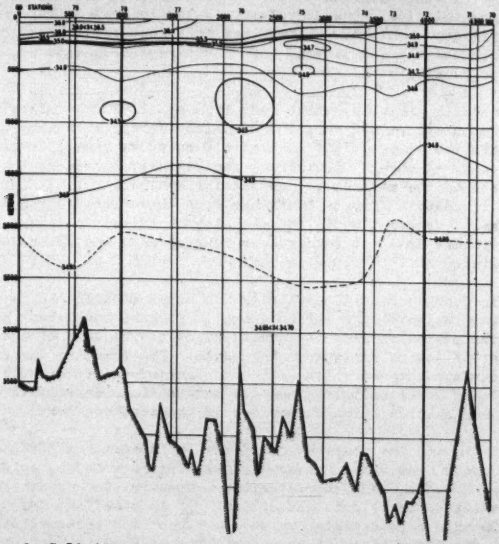

Fig. 6--Salinity-section based on Carnegie stations 70-80

At the surface we find an increasing salinity when departing from the coast. At the coast the lowest salinity is met with at the surface, where values below 34.5 o/oo occur. The water of this low salinity must originate from the Gulf of Panama where the surface-salinities are small on account of excessive precipitation. At some distance from the coast we find the highest salinity below the surface, but it can not be decided whether the high values indicate a transport of water towards the coast or is related to seasonal variations.

The second cross-section (Fig. 6) in about 15° south reveals principally the same features. The intermediate Antarctic Current can be followed across the whole section and the salinity within this body of water is now slightly smaller, areas with salinities smaller than 34.5 o/oo occurring. The axis of the Antarctic Current is again horizontal. At the surface we find a great increase of the salinity when departing from the coast.

In the third cross-section (Fig. 7) which runs from southeast to northwest between latitudes 40° and 28° we find the intermediate Antarctic Current still more strongly developed and still horizontal. The salinity has now values around 34.3 and values as low as 34.1 occur. At the right-hand side of the section we recognize the Peruvian Current at the surface where the salinity is about 34.0.

Turning next to the longitudinal sections we shall first examine the westerly section between longitudes 115° and 105° west and latitudes 25° and 5° south (Fig. 8). In this section we can follow the intermediate Antarctic Current as a horizontal flow. The decrease in salinity towards the north is plainly seen and it can hardly be doubted that this increase is due to admixture from above because the salinity of deep water remains unaltered, the curve of 34.6 o/oo running horizontal.

In the last section (Fig. 9) which runs from about 98° west longitude and 40° south latitude to Callao and cuts the Peruvian Current at a very small angle we find again the intermediate Antarctic Current very well developed with a minimum of salinity at about 700 meters. The salinity increases towards the north but the depth of the minimum values remains constant. It is furthermore evident that the increase is due to admixture from above because the salinity at 700 meters remains constant until the salinity of the water-layers above is appreciably greater and does not increase as long as only the underlying layers have a higher salinity.

Within the surface-current the salinity increases towards the north on account of the evaporation of the surface, but it remains much lower than the salinity at greater distances from the coast, evidently because of a constant transport of water of low salinity from the south. The Peruvian Current appears thus as a surface-current which is independent of the intermediate Antarctic Current with a lower salinity than the intermediate current in the most southern part of our section but a higher one in the northern part.

The low salinity and low temperature of the surface-waters off the Peruvian coast, as already mentioned, frequently has been regarded as evidence for the presence of an upwelling of the deep water. However, in our sections we find no feature which supports this assumption. If an upwelling from depths greater than 1,000 meters took place, we should expect the intermediate Antarctic Current to disappear close at the coast, but we find it just as well developed here as at greater distances. The horizontal distribution of the salinity at a depth of 700 meters (Fig. 10) shows a regular increase towards the

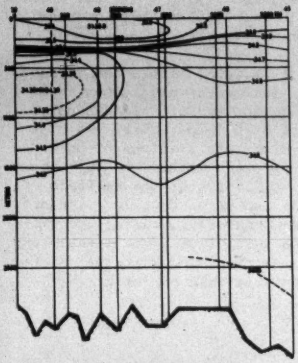

Fig. 8--Salinity-section based on Carnegie stations 45-50

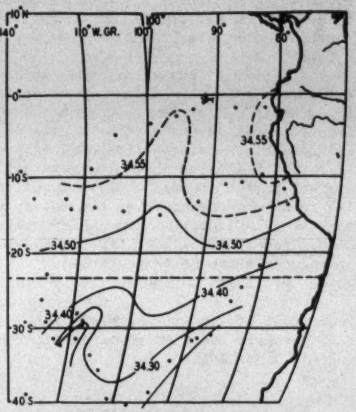

Fig. 10--Salinity-distribution at 700 meters

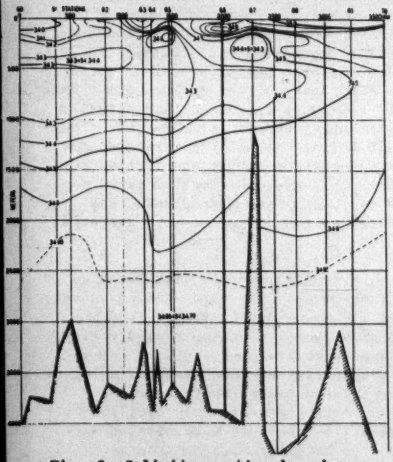

Fig. 9--Salinity-section based on Carnegie stations 60-70

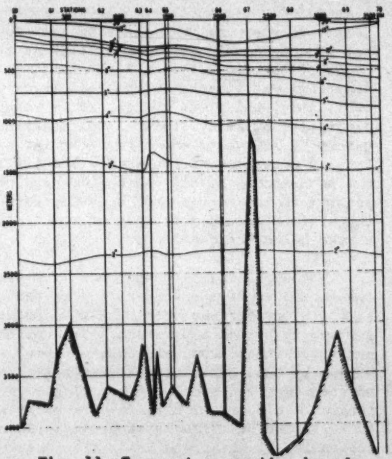

Fig. 11--Temperature-section based on Carnegie stations 60-70

north of the salinity of the intermediate current, indicating that this current is independent of the distance from the coast and effectively shuts off any communication between the deep-water and the surface-layers.

The possibility exists that an upward motion is found within the surface-current itself, when this approaches the coast. Such an upward motion would bring water of low temperature and low salinity to the surface because only the uppermost layers of the Peruvian Current have high temperatures and high salinities. An examination of a temperature-section from stations 60 to 70 (Fig. 11) indicates that such vertical motion is present within the Peruvian Current in the immediate vicinity of the coast. We find that the isotherm of 15° rises when approaching the coast, but all the isotherms from 10° down drop. This trend of the isotherms in my opinion must be regarded as conclusive evidence that the upwelling off the coast of Peru is a relatively small-scale phenomenon which is characteristic of the surface-current and that the water which is brought to the surface comes from depths less than 300 meters.

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SONIC DEPTH-FINDING ON THE CARNEGIE, CRUISE VII

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During the summer of 1927 while the non-magnetic vessel Carnegie was being overhauled prior to the beginning of her Cruise VII, sonic depth-finding equipment loaned by the United States Navy Department was installed. This equipment was of a type well suited for deep-sea sounding and consequently fitted the needs of the Carnegie. A Fessenden type of oscillator having a 30-inch steel diaphragm was located in the keel below the after part of the engine-room. This oscillator, which was the source of sound of 540-cycle frequency, was actuated electromagnetically, being supplied with alternating current of 540-cycle frequency at 180 volts and direct-current at 115 volts. A 5-kilowatt remote controlled motor-generator set for the alternating-current supply was located in the tool-room just off the engine-room on the port side; the control panel was located in the engine-room near the forward end. Six Navy hydrophones, any three of which could be used at one time, were located along the port garboard strake below the chart-room.

The depth-finder proper was located in the control-room (a deck-house on the port side of the forward end of the quarter-deck). The depth-finder acted as the clock for measuring the time required for the sound to travel from the surface to bottom and return. It consisted of a tuning-fork-controlled rotary converter which drove at constant speed a large bakelite disc. Riding on, and driven by this bakelite disc was a smaller accurately machined brass disc mounted on a splined shaft carrying a series of commutators which made and broke the electrical circuit of a relay which in turn operated the oscillator, thus sending out signals at periodic intervals. By means of a calibrated screw the radius at which the brass disc rode upon the bakelite disc, and consequently the time-interval between signals, was continuously variable between limits set by the dimensions of the bakelite disc. The outgoing signals and the returned echoes were audible in the telephone-receivers and in taking a sounding the position of the brass disc upon the bakelite disc was adjusted until the outgoing signals occurred simultaneously with the returned echoes of the immediately preceding signals. Under this condition, the time required for a signal to travel

to bottom and return was the same as the time-interval between two successive signals. A dial operated by the calibrated screw indicated, in effect, the latter time-interval.

A table, based upon an arbitrarily selected sound-velocity of 1,450 meters per second, was made for converting dial-readings into approximate depths, due consideration being given the horizontal distance between oscillator and hydrophones. As the velocity of sound in sea-water is a variable depending chiefly upon temperature, salinity, and pressure, the approximate depth was then multiplied by the suitable correction-factor selected from a table applicable to the area in which the sounding was taken. To the value thus obtained, a further correction for draft was applied.

As originally installed, the outgoing signal was brought to the receivers from the secondary of an air-core transformer, the primary of which was in the alternating-current circuit of the oscillator. Thus there would be heard, first the electrically conducted impulse of the outgoing signal, then the outgoing signal as a direct sound-wave picked up by the hydrophones, and finally the reflected sound-wave as picked up by the hydrophones. As the first two arrived but a short time apart, it resulted in a blurred sound of considerable intensity which had to be matched in time of arrival with a fainter sound of shorter duration. Later on the arrangement was changed and the air-core transformer eliminated, so that the outgoing signal was registered only as the direct sound-wave picked up by the hydrophones. This resulted in a sharper outgoing signal in the receivers, and a consequently greater ease and accuracy in getting a balance. After this change in arrangement a further constant correction of half the distance between oscillator and hydrophones was added.

The correction-factors applicable to a certain locality were grouped into a table of the ratios of the average velocity of sound down to the applicable depth, to the basic velocity of 1,450 meters per second, and were based on the British Admiralty Hydrographic Department Publication No. 282 entitled "Tables of the velocity of sound in pure water and sea-water for use in echo-sounding and sound-ranging." The variation in pressure at a given depth, due to the variation in gravity with latitude, was considered to be small enough to be disregarded. The range in temperature normally encountered is from -2° to +30° C while the salinity-range is within 31.00 to 38.00 parts per thousand. Correction-factors were computed for salinities of 31.00 and 38.00 parts per thousand and all even degrees of temperature from -2° to +30° C using Tables 2 and 3 of the British Admiralty publication cited above. From these factors a set of straight-line curves were drawn, one curve for each degree. While the isothermal variation of velocity with salinity is not linear, it was sufficiently so for this purpose.

Curves based on Table 1 below then gave the correction-factor to the basic velocity at any salinity and temperature at atmospheric pressure. The amounts to be added to the values derived from Table 1 because of pressure-effect, as taken from Table 4 of the British Admiralty publication, are shown in Table 2.

A set of correction-factors was prepared every two days, from actually measured temperatures and salinities, in the following manner. Vertical distribution-curves of temperature and salinity were plotted and from these curves were scaled the values at the nominal depths (in meters) of 0, 5, 25, 50, 75, 100, 200, 300, 400, 500, 700, 1,000, 1,500, 2,000, etc. From the velocity correction-curves, values of corrections were obtained for the conditions of temperature and salinity prevailing at the nominal depths. To these were added the corrections, due to pressure, corresponding to the appropriate

Table 1--Data used for graphs to determine correction-factors to basic velocity at any salinity and temperature at atmospheric pressure

Temperature	Salinity 31.00 o/oo		Salinity 38.00 o/oo	
	Velocity	$\left[\frac{\text{Velocity}}{1450} - 1\right]$	Velocity	$\left[\frac{\text{Velocity}}{1450} - 1\right]$
°C	meters/sec		meters/sec	
- 2	1430.96	-.0131	1440.04	-.0069
- 1	1435.68	-.0099	1444.72	-.0036
0	1440.30	-.0067	1449.30	-.0005
1	1444.92	-.0035	1453.88	+.0027
2	1449.34	-.0005	1458.26	+.0057
3	1453.76	+.0026	1462.64	+.0087
4	1458.08	+.0056	1466.92	+.0117
5	1462.20	+.0084	1471.00	+.0145
6	1466.34	+.0113	1475.08	+.0173
7	1470.38	+.0141	1479.06	+.0200
8	1474.32	+.0168	1482.94	+.0227
9	1478.16	+.0194	1486.72	+.0253
10	1481.90	+.0220	1490.40	+.0279
11	1485.56	+.0245	1493.94	+.0303
12	1489.12	+.0270	1497.38	+.0327
13	1492.68	+.0294	1500.82	+.0350
14	1496.04	+.0317	1504.06	+.0373
15	1499.30	+.0340	1507.20	+.0394
16	1502.52	+.0362	1510.38	+.0416
17	1505.54	+.0383	1513.36	+.0437
18	1508.56	+.0404	1516.34	+.0458
19	1511.48	+.0424	1519.22	+.0477
20	1514.30	+.0443	1522.00	+.0497
21	1517.04	+.0462	1524.68	+.0515
22	1519.78	+.0481	1527.36	+.0534
23	1522.42	+.0499	1529.94	+.0551
24	1524.96	+.0517	1532.42	+.0568
25	1527.50	+.0534	1534.90	+.0585
26	1529.92	+.0551	1537.28	+.0602
27	1532.24	+.0567	1539.56	+.0618
28	1534.56	+.0583	1541.84	+.0633
29	1536.88	+.0599	1544.12	+.0649
30	1539.00	+.0614	1546.20	+.0663

depth and temperature and taken from Table 2. The sum of these two corrections was entered in a column headed "Velocity-corrections" opposite the proper depth. The procedure for getting the correction-factors applicable to the various depths was from this point on a more or less obvious one of taking means. A specimen set of computations of correction-factors is reproduced in Figure 1.

Criticism may be made of the arbitrary selection of the basic velocity of 1,450 meters per second for the compilation of calibration-tables. Consideration was given to the selection of some velocity which would have some significance other than merely being the base for a set of tables. For instance, such as the velocity of sound in water of 35.00 o/oo salinity, 0° C temperature, and

Table 2--Amounts to be added to correction-factor because of pressure-effect

Depth	Temperature		Amount	Depth	Temperature		Amount
	From	To			From	To	
meters	°C	°C		meters	°C	°C	
25	-2	+25	.0003	500	-2	+5	.0063
50	-2	+25	.0006	1000	-2	+5	.0126
75	-2	+25	.0009	1500	-2	+5	.0188
100	-2	+25	.0012	2000	-2	+5	.0250
200	-2	+25	.0025	2500	-2	+3	.0313
300	-2	+25	.0037	3000	-2	+3	.0375
400	-2	+25	.0050	3500	-2	+3	.0437

Depth	Temperature °C				
	-1	0	+1	+2	+3
meters.					
4000	.0499	.0499	.0499	.0499	.0498
4500	.0561	.0561	.0560	.0559	.0559
5000	.0623	.0621	.0621	.0621	.0619
5500	.0684	.0683	.0683	.0681	.0680
6000	.0745	.0744	.0743	.0742	.0741
6500	.0806	.0805	.0803	.0801	.0800
7000	.0866	.0865	.0863	.0861	.0859
7500	.0926	.0925	.0923	.0920	.0918
8000	.0986	.0984	.0981	.0979	.0977
8500	.1046	.1043	.1040	.1037	.1034
9000	.1104	.1101	.1099	.1095	.1092
9500	.1162	.1159	.1156	.1153	.1150

atmospheric pressure. So far as could be learned, however, practice had not crystallized to the point of selecting such a velocity which could be considered as standard and as any velocity might be used equally as well as any other velocity it was considered best for the purpose to select a figure which was approximately a round number, was somewhere near the true velocity, and would give corrections which would be additive in nearly all cases. It was for these reasons that 1,450 meters per second was the velocity selected.

An estimate of the accuracy of each sounding was made and recorded at the time of the measurement. The method of arriving at these estimates may be of interest. The rotary converter and its controlling tuning-fork were of 60-cycle frequency. As long as synchronism was maintained the two had to maintain a phase-relation which was constant within a quarter-cycle, and it is probable that the successful synchronizing range was about one-eighth of a cycle. This meant that relative to the tuning fork, the rotating parts were varying in phase by a maximum of 1/480 second or about 3 meters in distance. As the distance traveled was twice the depth, the uncertainty in depth due to this cause was about 1.5 meters. As there was no temperature control or compensation on the tuning fork and as there was about 10° C range on either side of the mean, and as the tuning-fork rate had a temperature-coefficient of about 0.007 per cent per degree Centigrade, it was considered that the time-intervals indicated

SCRIPPS STA. NO: 98 LAT: 28° 9'.3 N. LONG: 167° 9'.4 W. DATE: 14 MAR 1928 CORR. FOR:

D	T_H	S	VEL. CORR.	MEAN CORR OF LAYER 25	100	500	SUM OF MEANS	CORR. FACT.	REMARKS
m	°	‰							
0	22.74	28.77	.0631				0.0631	1.0631	
5	22.76	28.68	—	.0633			0.1896	—	
25	22.73	28.76	.0631		.0634		0.4733	1.1633	
50	22.66	28.77	.0630	.0634			0.7367	1.0637	
75	22.10	28.78	.0625	.0629			0.1110	1.0618	
100	21.46	28.88	.0622		.0627		0.1748	1.0639	
200	19.70	29.05	.0576		.0601	.0195	0.2124	1.0626	
300	18.75	29.31	.0548		.0577		0.2701	1.0578	
400	18.25	29.79	.0546		.0547			1.0548	
500	17.9	29.97	.0523				0.1070	1.0525	
700	17.65	29.27	—			.0496	0.2791	—	
1000	17.7	29.98	.0510			.0533	0.3324	1.0544	
1500	17.7	29.82	.0525			.0587	0.4211	1.0587	
2000	2.2°	29.69	.0282			.0370	0.4581	1.0283	
2500	1.95	29.63	.0258			.0366	0.4827	1.0260	VELOCITY BELOW ASSUMED LIMIT
3000	1.75	29.65	.0250			.0423	0.5250	1.0275	BY EXTRAPOLATION
3500	1.60	29.67	.0242			.0382	0.5632	1.0254	
4000	1.45	29.68	.0240			.0417	0.6049	1.0279	
4500	1.20	29.67	.0247			.0473	0.6522	1.0270	
5000	1.10	29.67	.0222			.0453	0.6975	1.0272	
5500	1.0	29.67	.0218			.0716	0.7691	1.0270	
6000	1.10	29.67	.0244					1.0244	
6500									
7000									
7500									
8000									

Fig. 1--Specimen determinations of correction-factors

were subject to an error of 0.1 per cent. Further, there was an uncertainty of the dial-setting within which the outgoing and returning signals sounded as one to the operator. This uncertainty was converted into depth and if greater than 0.1 per cent and greater than 1.5 meters was recorded as the uncertainty of the measurement. If it was less than 0.1 per cent but greater than 1.5 meters, 0.1 per cent of the sounding was recorded as the uncertainty. And if the distance 1.5 meters was greater than both the uncertainty of setting and 0.1 per cent, then it was recorded as the uncertainty of measurement. It was thought that this was a reasonable procedure of estimating the accuracy of soundings. However, this is assuming that the frequency of the tuning fork was accurately adjusted to 60 cycles per second, that the sounding velocity used was accurate, that the sounding distance was vertical, and that no gross errors were involved. The conditions of temperature and salinity at nearby oceanographic stations are on record, and if in the future it is found that the velocities used were inaccurate, corrections may be made. Very often echoes would be reflected from more than one surface. In such cases the first echo to return was selected as being from that surface which was most nearly vertically beneath the ship. Because of the comparatively gentle slopes of the ocean-bottom, such a procedure is probably not greatly in error in soundings at sea, although it is recognized that in steep gradients, such as are encountered in certain approaches to land, the error may be considerable. Gross errors are possible when the returned echoes are matched with second or third succeeding signals instead of with the immediately succeeding signal, thus giving one-half, or one-third the actual depth. Such errors are easily avoidable by sending single signals in order to determine the order of magnitude of the depth. As the single signal was usually used to determine the number of reflecting surfaces and the number of echoes, there was little possibility of gross errors entering the Carnegie results from this cause.

A program of sounding every four hours was attempted. During such times as the ship was becalmed or making little headway soundings were taken about every ten miles. This program was, in general, followed, but in areas of rapidly changing depth more frequent soundings were made. Other deviations from this schedule sometimes occurred to avoid interference with pilot-balloon ascensions, or radio schedules, and occasionally due to the press of other work. Short interruptions to the sounding program were sometimes caused by the necessity of making repairs to the depth-finder or to the gasoline engine which drove the main generator. The longest and most serious interruption was caused by the failure of the oscillator on November 5, 1928. It was not until Callao

was reached that repairs to the oscillator could be made since such repairs required dry-docking. Consequently no accurate soundings were made between November 3, 1928, and February 6, 1929. Beginning November 14, 1928, rough soundings were made with an improvised shotgun. A steel breech just long enough to hold a 16-gauge shotgun-shell was screwed into one end of a length of brass pipe. The pipe acted as a holder and also as a guide for a heavy steel firing-pin which was dropped into the upper and open end of the pipe, the shell end being held a foot or two below the surface. The hydrophones were used to pick up the echo and a stop-watch used to measure the elapsed time. Soundings were taken in this manner twice a day. These were only roughly approximate because of the inaccuracy of the stop-watch measurement and because of the uncertainty of the velocity of a sound set up by an explosion. However, it was a case of half a loaf being better than none, and the device materially assisted in the routine occupation of oceanographic stations.

Aside from instrumental difficulties the most serious handicap to sonic depth-finding activities was that of hydrophone noises not the result of signals of their echoes. These noises were of two classes, those originating within the vessel and those due to turbulence of the water in the vicinity of the hydrophones. In the first class, the chief source of annoyance was the main engine. Except in the case of unusually loud echoes, it was impossible to sound with the engine running at full speed. Great difficulty was experienced even with the engine just turning over. The installation at Yokohama of two 500-cycle high-pass filters in the receiving circuit considerably reduced the noises originating from the engine. In fact, it was sufficiently effective so that echoes could sometimes be heard when the engine was running at full speed. It was far from a complete remedy, however, and seemed to have no effect on the noises due to turbulence of the water. Because of the location of the hydrophones and the depth of the Carnegie's keel, rolling of the vessel produced a turbulence around the hydrophones such that a veritable roar was produced in the receivers. Speed of the vessel also was a factor, beginning to produce water-noises at about eight knots and increasing until at about ten knots the noise was similar in intensity to that produced by a bad roll. Since the noises due to rolling were more or less intermittent and periodic it was possible to make accurate soundings by taking advantage of the quiet intervals. Noises due to the vessel's speed, however, were practically continuous and consequently represented a more serious interference. Fortunately, bad rolling usually occurred at the lower speeds.

The weakness of the returning echo was a puzzling element which sometimes made sounding difficult--puzzling in its seeming independence of slope, type of bottom, or depth. The intensity of the echo seemed to be related only to the frequency, but was indeed sensitive to that. At certain times a loud echo would be returned while at other times the echo would be extremely faint when all known conditions were similar and when bottom-samples at the two places seemed very much the same. While the slope of the reflecting surface was expected to have some effect on the intensity of the reflected wave and while it was thought that differences were detectable in passing some of the sharper irregularities, much of the ocean-floor, including areas of weakest and strongest echoes, has but a small gradient. True attenuation did not seem to be the cause of the weak echoes either. This was borne out by the fact that in our deepest soundings in Fleming Deep, where the depth was about five miles, the returned echoes were of greater intensity than in other cases where the depth was about 1500 meters. A further indication that attenuation alone did not explain the weak echoes was that in a number of cases where the depth was over 4,000 meters, two or more echoes would be audible. On several occasions the signal was picked up after traveling between 30 and 40 kilometers and one instance

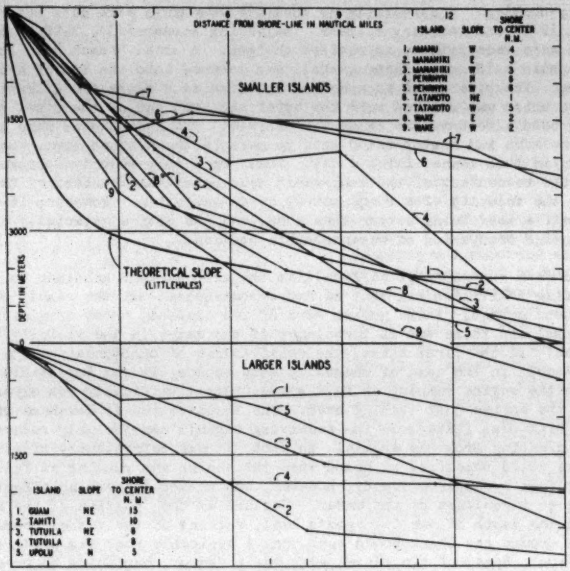


Fig. 2--Comparison of island-slopes

was recorded in which the sound was audible after traveling more than 78 kilometers during which it suffered 15 reflections.

In all 1,500 soundings were made (record for one of these was lost in the destruction of the Carnegie. Of this number 115 were made with the shotgun, and in 1,385 the oscillator was used. The average separation of the soundings was about 30 sea-miles. Some of the profiles of approach to land in the Pacific are shown in the accompanying diagrams. These are all islands and are therefore shown along with the maximum slope which is theoretically stable according to Littlehales (Bull. Nat. Research Council, No. 17, 90-93, 1922). Inasmuch as some of these islands have an appreciable mass above the water-level, a strict comparison is probably not justifiable. In order to better compare the actual bottom-slopes, all the curves have been started from the shore-line and the distance of the center of the peak from the shore-line has been given in tabular form to enable the reader to differentiate between the large and small islands.

Of the nine islands the approaches for which are shown in Figure 2, four are grouped as large and five as small. An interesting feature which offers food for speculation is that the four small islands shown in Figures 3, 4, and 6 have a secondary ridge or elevated prominence on their northeastern sides. In the case of Penrhyn Island, this ridge apparently comes very close to the surface and is known as Flying Venus Reef. The absence of similar ridges on

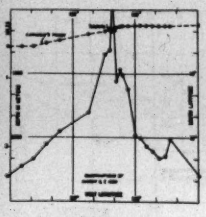


Fig. 3--Profile Tatakoto Island

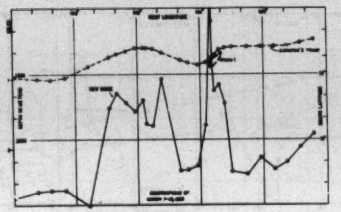


Fig. 4--Profiles Amanu Island and unnamed ridge

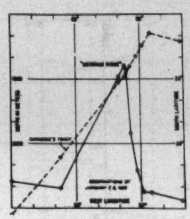


Fig. 5--Profile Merriam Ridge

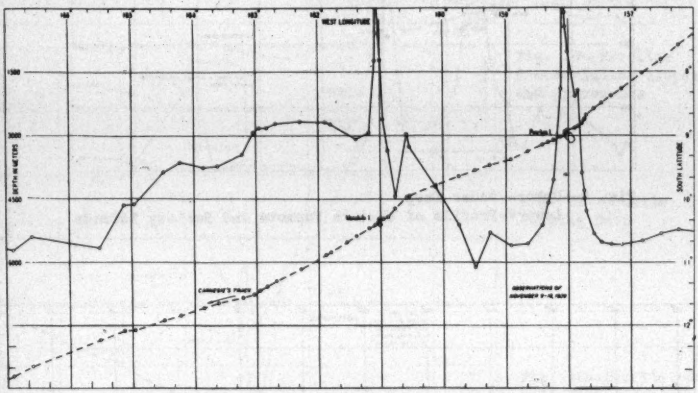


Fig. 6--Profiles Manahiki and Penrhyn islands

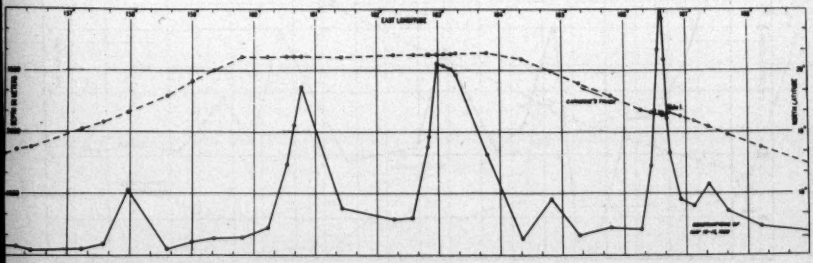


Fig. 7--Profile of and bottom-irregularities near Wake Island

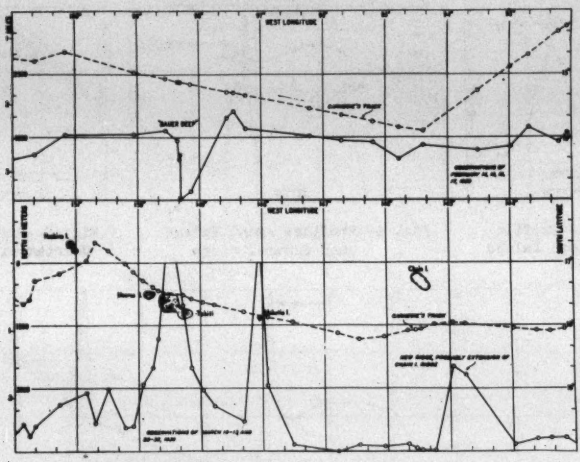


Fig. 8--Upper--Bauer Deep
Lower--Profile of Western Tuamotu and Society islands

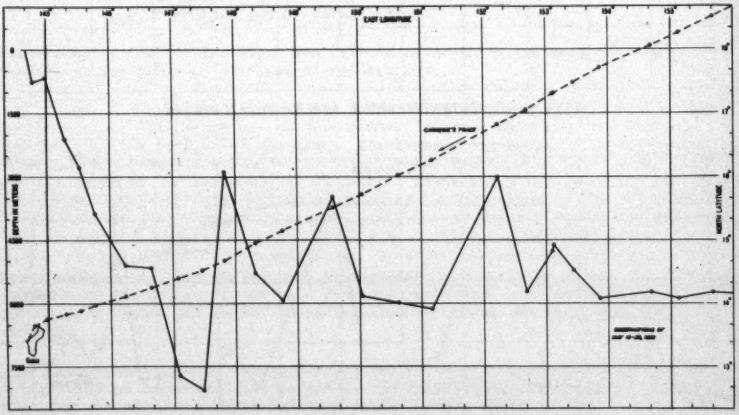


Fig. 9--Profiles Guam Island and approach

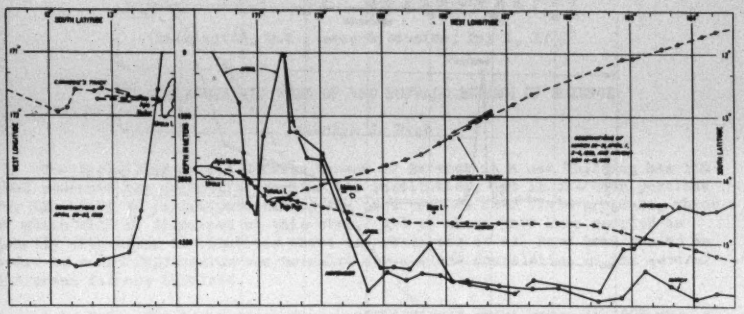


Fig. 10--Profiles Samoan-Island slopes and approaches

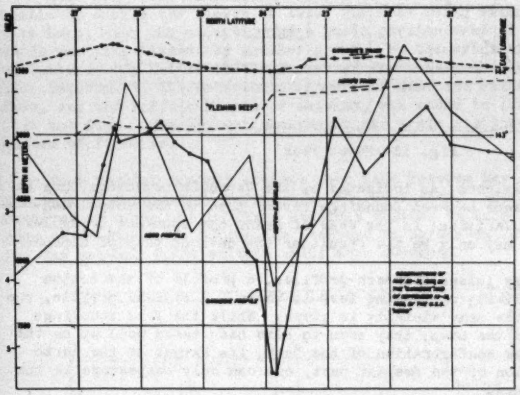
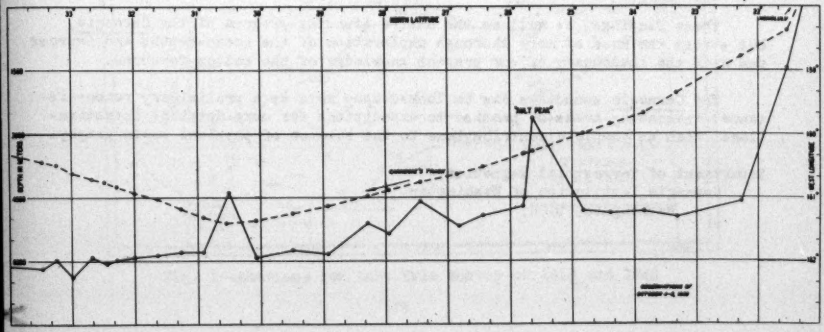


Fig. 11--Profile Fleming Deep

Fig. 12--Profiles Ault Peak and approach to Oahu Island



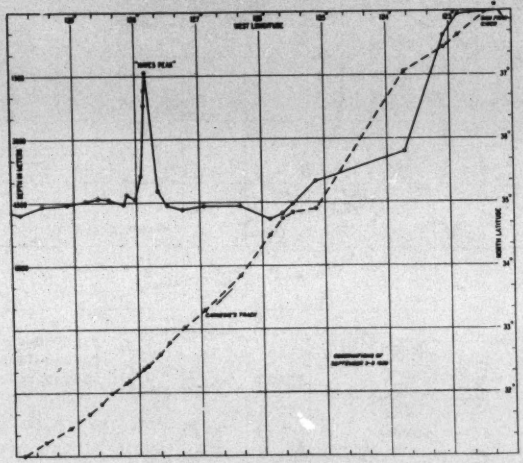


Fig. 13--Hayes Peak

the other sides of the islands, as indicated by the Carnegie soundings, may be due to too great a distance between soundings rather than to the actual non-existence of such irregularities; in the case of Amanu the apparent irregularity may not be real and may only be the result of the devious path of approach.

In addition to these island approach-profiles, a profile of the bottom north of Guam in the vicinity of Fleming Deep is shown. A similar profile, run by the J.S.S. Nero, in the same vicinity is given. While the Nero soundings hint at the existence of the Deep, they seem to have been taken well up on the western slope. As to the configuration of the Deep, its extent to the north and east, and the location of the deepest part, one can only conjecture in the absence of other soundings.

These findings, as well as the entire sounding-program of the Carnegie, but stress the need of more thorough exploration of the ocean-depths and impress one with the inadequacy of our present knowledge of the bottom-features.

The Carnegie soundings can be looked upon more as a preliminary reconnaissance, indicating areas of promise to expeditions for more detailed investigations, than as complete contributions to the science of physical oceanography.

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SECTION OF OCEANOGRAPHY

(Washington, D.C., second session, May 2, 1930)

THE OCEANOGRAPHIC WORK OF THE BUFFALO MUSEUM OF SCIENCE

Charles J. Fish

The installing of the Buffalo Museum of Science in a new building has limited somewhat the scientific work of the institution, but it has been possible for the staff to participate during the past year in four field programs, three of which will be discussed at this time. All of these have been carried on jointly with other institutions where the resources of all have been pooled in order to avoid duplication and to allow for a close correlation of the several different factors involved.

Lake Erie--The Cooperative Survey of Lake Erie which began in 1928 was continued during the season of 1929, the area being extended to cover the entire Lake, with the exception of a small portion west of the islands in Ohio which was investigated by another group. To the original five cooperating institutions, the United States Bureau of Fisheries, the New York State Conservation Department, the Ontario Department of Game and Fisheries, the City of Buffalo, and the Buffalo Museum of Science, was added in 1929 the Ohio Division of Fish and Game. The steamer Shearwater was again assigned by the United States Bureau of Fisheries.

Four regular monthly cruises were made between May 15 and September 20, starting at Buffalo and terminating at Put-in-Bay. Approximately 15 days were required to cover the 50 fixed stations visited on each cruise. In all, 250 regular stations and 20 special stations at harbor-entrances were occupied during the season; 4,435 statute miles were covered.

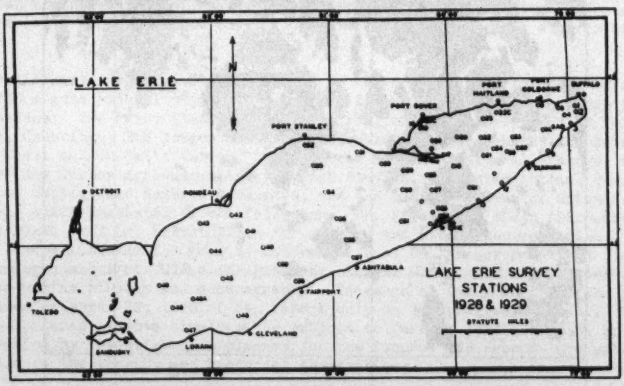

Fig. 1--Stations for Lake Erie Survey of 1928 and 1929

As the work of the first season (1928) was reported on in considerable detail at last year's meeting and the results published in the "Bulletin" (v. 14, 7-220, 1929) of the Buffalo Museum, it will suffice to say that the results in 1929 corroborate in almost every detail the tentative conclusions advanced at that time. For this reason a detailed discussion of data in terms of the objects of the Survey will not be repeated.

It may be well, however, to mention one aspect of the problem which was completed during the second year. Owing to the limited area covered in 1928, it was not possible to determine the western boundary of the cold bottom-water (6° C or less), and only the oscillations of the eastern margin were ascertained that season. In 1929, during the period from May to September, the cold mass was found to be confined to a large somewhat oval-shaped area in the deepest part of the Lake. In May the cold water occupied a region approximately 55 miles long and 30 miles wide, the extreme range of the oscillations extending roughly from Erie to the longitude of Dunkirk. Elsewhere over the greater part of the Lake the water responds to the rising air-temperature and in summer contains no thermocline.

Westerly winds raise the Lake level at the eastern end, and depress the cold bottom-water to the westward. Similarly, easterly winds lower the level at the eastern end, and the bottom-water movement, in the adjustment that follows, is to the eastward. A report on the two years' work in Lake Erie will soon be ready for publication.

Gulf of Maine--During the past summer the Museum joined with Brown University and the Mt. Desert Biological Laboratory in an investigation of the neritic waters of the Gulf of Maine.

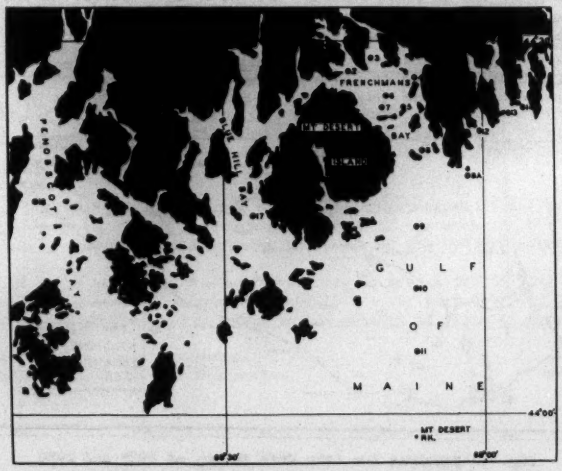

Fig. 2--Stations for Gulf of Maine Survey of 1929

The major object of the investigation was to study the relative chemical and biological contributions of the various outwash areas in the vicinity of Mt. Desert Island, and the natural economy in the marginal region of the Gulf. The area covered extended from Penobscot Bay east to Petit Manan, with one section extending from Frenchman's Bay to Mt. Desert Rock. Forty-nine stations were visited during July and August, and investigations carried on on the hydrography, chemistry of the water, phytoplankton, and zooplankton. Chemical determinations consisted of phosphate, nitrate, nitrite, silicate, and dissolved-oxygen analyses.

Inasmuch as the work was of a preliminary nature and will be continued by a larger staff this season, a formal report will be withheld until the investigations have been completed. This seems particularly advisable in view of the fact that the past summer proved to be an unusual one. There was very little rain during the period of the observations, and both the plankton fauna and the salinity-values in the inner part of Frenchman's Bay approximated those of the open Gulf. The marked variability in the chemical results and the sporadic occurrence of local diatom-maxima on at least two occasions during the midsummer will require further investigation. A new vessel has been acquired by the Mt. Desert Laboratory and the joint staff of seven members will begin work on June 15.

International Joint Survey in the Bahamas--For several years investigations of the marine sediments of the Bahama Banks, particularly the calcium-carbonate deposits found on the west coast of Andros Island, have been carried on by Dr. Richard M. Field of Princeton University and his associates. Inasmuch as apparently similar deposits are found in several geological periods, the origin of these sediments and the conditions governing subsequent changes in their character are of particular interest to the geologist as well as to the oceanographer.

A theory has been advanced that the particular type of fine calcium-carbonate mud deposits known as drewite are of organic origin and are being formed at the present time. An excellent opportunity was therefore afforded for correlated studies on the physical, chemical, and biological aspects of the problem.

Preliminary investigations were made on two occasions in 1928 and in January 1930 a more extensive joint program was begun by the following group of institutions: The Percy Sladen Trust of London; the Rouse Ball Fund of Trinity College, Cambridge; Die Notgeminschaft der Deutschen Wissenschaft; the United States Coast and Geodetic Survey; the American Museum of Natural History; the State of New Jersey Agricultural Experiment Station; the International Summer School of Geology and Natural Resources; and the Buffalo Museum of Science. The field staff consisted of the following: Dr. Richard M. Field (Director), Maurice Black, and Dr. Ernest Dixon for geology and marine sedimentation; Lieut. Joseph Luschene for gravity-determinations; Dr. Werner Bavendamm for bacteriology; and Prof. Ulric Dahlgren, Dr. Roy W. Miner, and Dr. Charles J. Fish for marine biology and oceanography. Inasmuch as field work for the year terminated on April 20, 1930, I can report only on the progress to date. The opinions offered at this time must be subject to further substantiation or modification by investigations planned for the future. No records are yet available on the gravity and bacterial observations.

There are two possible sources of the drewite on the shoals west of Andros. Either the sediments are being formed there in tremendous quantities, or else they are washed from the land and reworked by the sea. The program therefore has been carried on in two parts: First, a determination of the

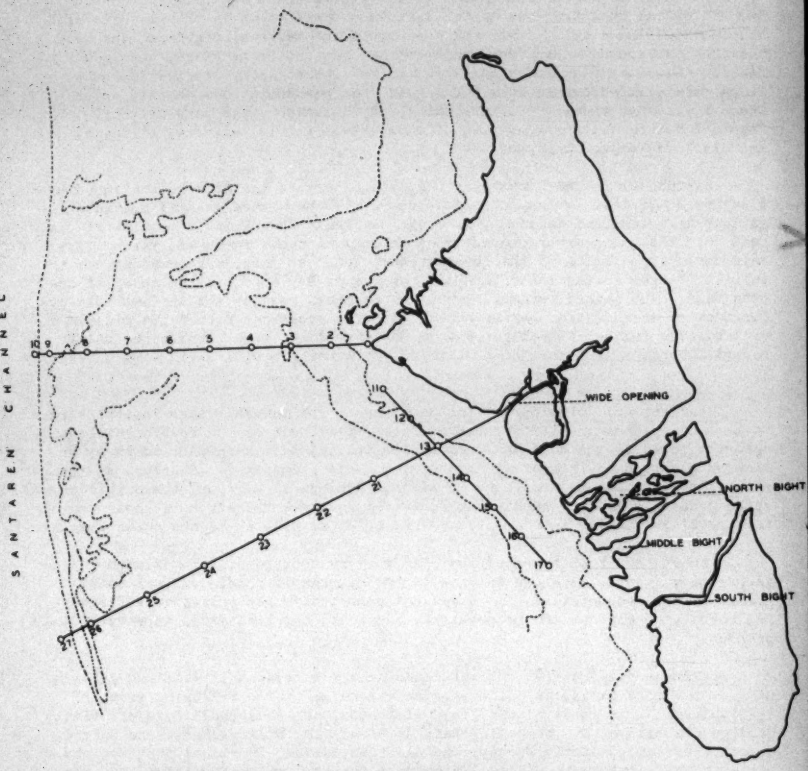

Fig. 3--Sketch-map Andros Island and shoals west, Bahamas

physical, chemical, and biological conditions on the shallow banks; and, second, a study of the geology of Andros and the neighboring islands of the Bahama group.

Oceanological observations have to date consisted of temperature, salinity, pH-determinations, bottom-sampling for sediments and the fauna of the mud, bacterial analyses, bottom-dredging, and plankton-collections. This year two sections were run from the west coast of Andros to the Santaren Channel, and one parallel to the coast from the vicinity of William's Island to Middle Bight. Over the entire area investigated the depths do not exceed four and one-half fathoms, and during storms so much sediment is held in suspension that the water becomes milky white, often remaining in this condition for several weeks at a time.

Probably the most significant biological characteristic of the region is the comparative scantiness of the fauna. Unfortunately there are no available data on conditions before the series of hurricanes which have occurred during the past few years. Undoubtedly some of the benthonic forms have been materially reduced in number, but, considering the unusual environmental conditions in the region and the especially adapted forms which characterize it, a scanty fauna might be expected even under the most favorable weather-conditions.

Of the two available sources of support for neritic life in this area, the land and the Gulf Stream, the former, consisting of low, barren wasteland, can obviously contribute comparatively little to the fertility of the waters. The Gulf Stream would offer a much more favorable source of supply. However, the pelagic fauna, except near the margin of the Santaren Channel, was found to be quite neritic in nature, and the cursory examinations made so far have revealed no indication of Gulf Stream influence.

None of the species dominating the deep-water community appear to have invaded the shallow zone over the banks. As the collections were made under very favorable weather-conditions it is not possible to definitely state to what extent continued westerly winds may transport surface-forms on to the shoals. However, considering how well the pelagic fauna of the Gulf Stream elsewhere is normally restricted along its landward margin, it is reasonable to assume that any influx on to the shallow banks off Andros would be sporadic and probably exert little, if any, permanent influence on the local fauna, if the limited collections of the past two years prove indicative of normal conditions. The poverty of life in the region and the apparent lack of a source of support for more than a scanty fauna appear significant in considering the possible organic origin of the drewite.

The restricted distribution of the deposits renders it difficult to explain the formation of drewite on the basis of biological or chemical action in the sea. In the area under consideration it occurs as a band of approximately the length of the island, extending from the shore almost to the Santaren Channel where it is replaced by coarser sediments. North and south of this band there are areas of lime sand. Bottom-cores indicate that in a typical section the deposit is thickest near the shore and gradually tapers out into a layer so thin that in the outer part of the bank the basal limestone practically lies exposed. As the basic rock is not level, the gradation is not entirely even along the whole coast. There are depressions filled with drewite and some slightly raised areas of bed-rock. Were the drewite being formed by organic means the thickest part near the shore would indicate a concentration of activity in that zone, but as yet no supporting evidence has been found. The richest benthonic life in the region occurs where the drewite is least abundant. It is still more difficult to explain such unequal deposition by chemical means. The distribution of the sediments clearly suggests outwash from the land, particularly since all drewite-areas reported in the Bahamas are situated on the leeward side of the islands.

The geological investigations on Andros were carried on over a period of three months. Foremost among the problems investigated was the stratigraphical position of the drewite-deposits on the Great Bahama Bank, and their relationship with the other sediments in this area. The unconsolidated strata of the marl-area of Andros were studied first, and an attempt was made to determine the nature of their contact with the underlying limestone by running a series of traverses across the island. Except at the margin of the bank, it was found that the surface of the solid limestone maintained a remarkably constant level over the whole area, the maximum relief being not more than 35

feet. The hollows in the rocky platform are partially filled by drewite, whilst the elevations stand out as limestone-ridges. Nowhere were the unconsolidated sediments found to be of any great depth; the greatest thickness of drewite encountered was 22 feet at Purser Point. From the Wide Opening the drewite thins to the west, and rocky outcrops appear at the surface west of a line joining the eastern end of the White River with the western end of Sbuth Bight. Thus, the drewite-deposits south of the Wide Opening are in the shape of a broad, flat wedge, and critical section of the contact between the deposits and the underlying limestone can only be obtained at the margin of this wedge, or where knolls of limestone rise through the drewite.

The beds overlying the basal limestone do not possess the characteristics of a marine sediment, but present distinct indications of fresh-water deposition. There is a basal conglomerate consisting of fragments shaped by solution rather than by attrition, and the overlying drewite contains terrestrial fossils exclusively. This suggests that the marl of Andros was, over a considerable area, deposited under fresh-water conditions and not on an open marine shelf. Examination of the drill cores from the thickest parts of the drewite-wedge will be required to either substantiate or disprove this suggestion.

Apart from the chemical uniformity of the materials involved, the sedimentation of the Andros region appears to be complex. In the normal course of erosion, mechanical processes are subordinate, and the principal agents at work are solution and chemical disorganization of the rocks. During brief periods of violent storm, however, the relative importance of these two agencies is reversed, and powerful mechanical erosion takes place unaccompanied by any appreciable chemical alteration. During hurricanes extensive rearrangement of previously deposited sediments takes place with remarkable rapidity. Since the forces which determine the distribution of transportable sediments during hurricanes may be entirely different in their direction and power from those at work during normally calm periods, it is necessary to disentangle the results of storm deposition and quiet-water deposition before a true picture can be obtained of the effect of either process individually.

It is the slow chemical changes which requires investigation most urgently, for a brief survey of several areas in the center of North Andros has revealed conditions which suggest that weathering of the basal limestone may have much to do with the origin of drewite.

Local conditions, much more than the original nature of the rock, seem to govern the course which weathering will take, so that a comparatively homogeneous parent-rock will produce a series of new sediments with a surprising variety of texture and hardness. In the interior of Andros, where large areas of limestone are exposed to the water of ill-drained seasonal swamps, there are indications that bodily disorganization of the limestone produces a fine-textured material which, on being removed and redeposited, might form a drewite-like sediment. It is hoped that the material collected will throw some light upon this question.

Thus, the oceanographical evidence to date suggests that the drewite-deposits on the submarine parts of the banks may represent reworked sediments derived from the land, and that the present neritic fauna is a resultant, rather than a causative, factor in the formation of the lime deposits. The geological investigations on Andros indicate that drewite may result from the chemical action of water on older sediments above sea-level, thus affording additional evidence that the present submarine deposits of this material may be of non-marine origin.

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Buffalo, New York.

HYDROGRAPHIC WORK OF THE UNITED STATES COAST AND GEODETIC SURVEY

W. E. Parker

The hydrographic surveys by the United States Coast and Geodetic Survey are becoming yearly of greater and greater value to the science of oceanography. This is due to two reasons, the surveys are being extended further seaward each year and the accuracy of these surveys is increasing steadily from year to year. Modern navigation demands this and modern appliances and methods make it practicable. The science of oceanography is benefited thereby.

Before the days of echo-sounding, the mariner was but little interested in ocean-depths so long as they were sufficient for the safe passage of his ship. True, he took a few fairly deep soundings, or was supposed to do so, to enable him to determine his distance offshore and to aid him in approaching port when forced to navigate by dead-reckoning. This was a tedious and time-consuming operation and, accordingly, was resorted to only when very much in doubt as to the position of his ship. Navigation by means of soundings alone was practiced by only the most skillful and progressive shipmasters.

Hydrographers, therefore, looked upon offshore surveying as work that could well be deferred until after the shallow waters close inshore and in the bays, sounds, and other navigable areas had been thoroughly explored. Offshore shoals and banks were examined to such extent as was practicable with existing means but more for the purpose of discovering and charting dangers to navigation than to develop the topography of these elevations of the sea-bottom. Consequently, few soundings had been taken in the outer parts of the continental shelf and very few indeed beyond the shelf.

Not only were there few soundings, but those few were of doubtful accuracy. For many years, it has been possible to measure, with reasonable precision, even great ocean-depths, but to determine even approximately the geographical position of such measurements has been extremely difficult. Under the most favorable circumstances, a sounding taken 50 miles offshore might be located within three or four miles of its true position and this is perhaps the measure of accuracy of the best of the offshore hydrography executed prior to the last decade. It is doubtful whether the charts of the oceans of the world, except those of very recent date, show many soundings far offshore which have been fixed in position with anything like this accuracy.

This means that adjacent soundings may be at the distance apart indicated by the chart or they may be a fraction of or many times that distance. If the soundings were taken by the same ship while cruising on the same course, they are probably correctly shown with reference to each other, but if taken along different tracks, even by the same ship, they may be greatly in error with reference to each other. It is, therefore, evident that one should not place much confidence in conclusions as to bottom-slope or configuration that are based solely on the information shown on the older charts of offshore areas.

Echo-sounding has not only created a demand for accurate charts of deep waters, as a means for navigating such waters but has provided one of the means by which such charts can be made. Soundings can be taken by echo-apparatus about as accurate as by wire-measurements, except on steep slopes and over broken, rocky bottom--conditions not usually found in deep water. And since echo-soundings can be taken while steaming at full speed, the principal sources of error in hydrography controlled by dead-reckoning, namely, variable rate of patent logs at slow speed and drift of vessel while stopped for wire-measure-

ments, are eliminated. Thus, the geographical location of echo-soundings is far more accurate than of wire-soundings and, of course, the area that can be covered and the frequency of soundings taken in a given time are very greatly increased.

Following closely the adoption of echo-sounding has been the development of sub-aqueous sound ranging as a means for fixing the greater accuracy of the position of soundings taken beyond the range of visibility of the land. By this means, the geographical position of the surveying vessel can be determined, at any time, with a high order of accuracy, even when over a hundred miles from land. This means that soundings taken more than a hundred miles from shore can be located with all the precision required for the highest order of navigation and, it is believed, for the most complete and accurate study of the ocean-bottom.

The Coast and Geodetic Survey has been engaged in echo-sounding and sub-aqueous ranging for about six years, during which time the methods and instruments have been steadily improved and their application enlarged. Beginning on the Pacific coast of the United States, where there was at that time the greatest need for new methods, both of these methods of surveying have been extended to other localities. Echo-sounding is now used extensively in the surveys and resurveys of the Atlantic coast of the United States and in the surveys of Alaskan, Hawaiian, and Philippine waters; in fact, everywhere except in very shallow waters, where the highest degree of accuracy is required, and in a few places where the slope and bottom-configuration are such that the requisite accuracy cannot be obtained by this method. Sound-ranging presented certain difficulties, which at first seemed insurmountable when shallow depths extend more than a few miles offshore. Just recently a solution appears to have been found for these difficulties, so that it may be possible to use this method anywhere. It is now in general use in Western Alaska and in use experimentally on the Atlantic coast.

Ten years ago the charts of the Pacific coast of the United States were far from adequate, except perhaps those of the approaches to the principal harbors. Since then, more than three-quarters of this coast has been resurveyed accurately and completely out to depths of at least 1,000 fathoms. The most difficult of this work has been accomplished; the remainder can be accomplished easily within the next three years.

Similar progress has been made in Alaska during the last ten years. The coast of Southeastern Alaska has been thoroughly surveyed out to a depth of 1,000 fathoms from the international boundary in Dixon Entrance to Lituya Bay--practically all of the coast of Southeastern Alaska. In Southwestern Alaska large areas have been surveyed between Prince William Sound and Cook Inlet, in Shelikof Strait, and along the southern side of the Alaska Peinsula. It has not been possible to carry this latter work beyond depths of a few hundred fathoms because of the urgent demand for charts of the inshore waters, bays, and other indentations, but it is expected that, as soon as these demands have been satisfied, surveys will be extended to depths of 1,000 fathoms everywhere in Southwestern Alaska.

Hydrographic surveying was resumed in Hawaiian waters four years ago when a comprehensive plan was adopted for the completion of the survey of the coasts of the islands and of the insular shelf. The program was later expanded to include also the deep waters adjacent to the Archipelago. Substantial progress has been made on the western side of Hawaii, around Kauai and Niihau, in the waters between these latter islands and Oahu, and westward of Kauai as far as

French Frigate Shoal. An area of 50,000 square miles was surveyed during last year.

In the Philippines, the work has progressed steadily. Five years ago, after a careful study of the situation, a new program of field operations was adopted so as to insure the completion of the survey of the island waters in the shortest possible time. Here weather-cycles are a factor of more than usual importance in the efficient planning of hydrographic work. Under this program the work is rapidly approaching completion, the most difficult and dangerous part has been completed, and there is every reason to expect rapid progress on the remainder. Approximately 85 per cent of the waters have been surveyed since the Coast and Geodetic Survey took over the work 29 years ago. On completion of this task, attention should be given to the deep waters adjacent to the island group, especially to the great depth which lies to the eastward of the Island of Mindanao,and to the shoals, notably Scarborough Shoal and Pratas Reef, west and northwest of Luzon.

Progress in the resurvey of the Atlantic and Gulf coasts of the United States has been seriously handicapped by lack of suitable ships and funds available for that use. Happily, this situation will materially improve within the year. A fine large yacht, quite suitable for survey work, was donated to the Bureau last winter and a ship designed especially for surveying will be completed before the end of the year. With these ships added to the existing fleet and with the increased appropriation authorized for the next fiscal year, good progress may be expected hereafter along the Atlantic and Gulf coasts. To date, this coast has been well surveyed out to depths of 100 fathoms from Cape Lookout to Miami, except for a gap between Cape Canaveral and Palm Beach, which probably will be completed this spring. Similar work has been done off the entrance to New York Harbor, the Delaware and Chesapeake Bays, along the Gulf coast from Pensacola to the Mississippi, and in the approach to Port Arthur, Texas, except that on the Gulf coast, the work does not extend to depths of 100 fathoms. The present program contemplates new surveys along the entire Atlantic coast to the edge of the continental shelf and along the Gulf coast to depths of 100 fathoms except where this contour lies very far offshore. The most important offshore work of the near future is a projected resurvey of Georges Bank, planned for next summer. This Bank has an area of about 10,000 square miles, of which perhaps 2,000 square miles have a depth of ten fathoms or less with a least depth of about two fathoms. Accurate knowledge regarding the Bank is of outstanding importance to shipping between the North Atlantic states and Europe, and of equal importance to the fishing industries of New England. It may also be of no little importance to the science of oceanography.

It should be understood that the work of these hydrographic parties is by no means limited to getting the depths and the accurate positions of those depths. It has always been the practice to examine and to report the nature of the bottom, which information is given on the charts published by the Bureau. Also samples of the bottom-material are preserved when desired by some other organization for analysis and study. Temperature and salinity of the sea-water were observed rather infrequently prior to echo-sounding, but, with the adoption of this method of sounding, we were compelled to study these properties of water. In fact, the accuracy of echo-soundings depends upon the adoption of a current velocity of the sound-wave, which varies with temperature, pressure, and salinity. Accordingly, instructions for sounding by the echo-method require that "temperature-observations of the sea-water shall be made at a sufficient number of places and depths (depending on the variation of temperature from place to place and at different depths at the

same place) to insure that the estimated mean temperature for each sounding will not be in error by more than 1° C." The requirement for salinity-observations is "salinity-observations shall be made with sufficient frequency to establish the salinity at each sounding within one part in 1,000."

All of this means to the oceanographer that the continental shelf of the United States and the insular shelves of the dependencies and even the deep waters for a short distance beyond the shelves are being surveyed accurately and at a good rate of progress and that rather complete information is being gathered relative to the nature of the bottom, and the temperature and density of the sea-water.

U. S. Coast and Geodetic Survey,
Washington, D. C.

SURFACE TEMPERATURE, DENSITY, AND SALINITY PROFILES ACROSS THE STRAITS OF FLORIDA

Charles F. Brooks

Published oceanographic sections

The oceanographic sections across the Straits of Florida have been too restricted as to points of observation and distribution through the year to reveal more than general features (references 1, 2, 3, 4, 5, and 6). These features in February and March, diagrammatically summarized by Wüst (reference 7), are as follows:

Temperatures--The water is warm, over 10° C, to depths of about 200 meters on the Florida side and of about 600 meters on the Cuban and Bahamas margin. It is very warm, over 20° C, in the upper 50 or 100 meters near the Florida shore and 200 meters on the south or east. A 100-meter surface-layer with a temperature exceeding 25° C occurs on the Cuban half of the Straits. Temperatures at all depths are some 5° to 10° higher on the Cuban and Bahamas sides than on the Florida side. At the surface, however, the difference averages less than 5°

Salinities--The body of rather saline water, over 35 o/oo, extends to depths of from 200 to 400 meters on the Florida side and 600 to 700 meters on the Cuban and Bahamas sides. Of greater salinity, over 36 o/oo, is the upper 100 to 400 meters, this saline layer being thinnest on the Florida side, and, in fact, not reaching the Florida coast at the northern extremity of the Straits. A layer of still greater salinity, over 36.5 o/oo, the undiluted contribution of the Sargasso Sea, comprises the 50 meters of water from 150 to 200 meters below the surface, except near the Florida side, where it thins and comes to an offshore ending at a depth of about 100 meters. Surface-salinities are a little above 36 o/oo, being highest near Florida and lowest a little south or east of the middle of the Straits.

Thermograph-profiles

The occurrence of marked contrasts of surface-temperature in short distances, except near shore, became known only with the installation of a sea-water thermograph on a ship crossing the Straits. Since a full account of these automatic records is being published elsewhere (reference 8), only a brief summary of their outstanding features will be included here.

Springtime profiles (see Fig. 1) reveal a rather abrupt change from the cooler waters off the Florida reefs to the warmer of the middle and Cuban or Bahamas side. The lowest temperature, in fact, is found at times sharply defined on the seaward margin of the cool band. Abrupt changes of several degrees are sometimes recorded here within a distance of one or two nautical miles. Other differences in temperature, which would be ascribed to errors of observation if not automatically recorded, occur irregularly.

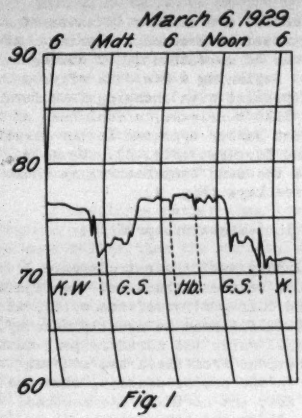


Fig. 1

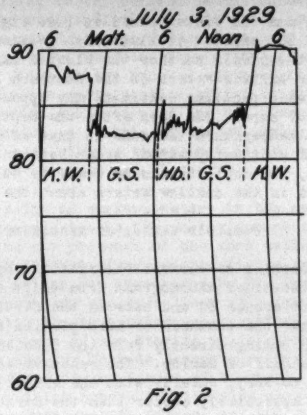


Fig. 2

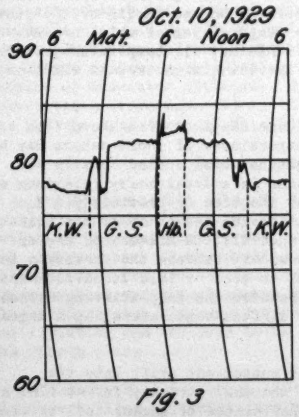


Fig. 3

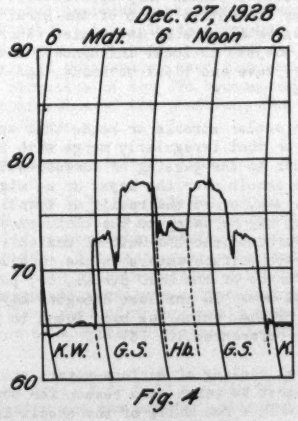


Fig. 4

Figs. 1-4—Typical sea-water profiles across Straits of Florida for spring, summer, fall, and winter

Summer profiles (see Fig. 2) are generally rather uniform, though variations within limits of one or two degrees are not uncommon. Temperatures are generally a fraction of a degree higher on the Key West than on the Cuban side. Furthermore, a narrow coastal band of water a degree cooler than the general body often appears on the Cuban margin. Daytime temperatures are usually higher than the nighttime, often by one degree even at the recording depth of about 2 meters.

Autumn and winter profiles (see Figs. 3 and 4) are similar to those of spring, but exhibit at times much greater contrasts than occur in spring. After a strong cold norther the Florida margin may be more than 10° C colder than the surface-waters of the southern half. Beginning a few days after a strong wind peculiar contrasts may appear and persist with changing form for a week or more. Ten days after the September 1929 hurricane, a cool band of uniform temperature the same as that of Key West Harbor appeared in the Straits south of distinctly warmer water between it and Key West (Fig. 3). Usually, however, the cool streaks or bands do not have the same temperatures as those observed in the shallow waters about the Florida keys (Fig. 4).

Possible causes of irregularities in temperature-profiles

According to current-observations by Pillsbury and others (reference 1) and to sea-water thermograms from ships en route between Habana and the Panama Canal (reference 9) and between the Straits and Gulf ports (reference 10), it seems that the southern or main portion of the Gulf Stream is usually made up of water coming directly from the Yucatan Channel while the northern part comes from the Gulf of Mexico. The relative contributions from these two sources appears to vary, chiefly with the wind. Thus in the cooler seasons, when the Gulf is appreciably cooler than the Caribbean Sea, the northern and southern portions of the Gulf Stream exhibit distinctly different temperatures, and their relative widths vary appreciably. Since the Gulf Stream does not usually occupy the entire width of the Straits, there is on one margin or the other, sometimes both, more or less quiet waters the temperatures of which are decidedly subject to local influences, namely, sunshine, air-temperature, wind-stirring, wave and tidal movement, and local upwelling with certain winds or currents.

The cooler streaks or bands that appear some distance from shore (see Fig. 6) or that irregularly merge with the temperatures of shore-waters may be due either to the passing of surface-water that has been cooled locally as over the shoals near the keys, or as might occur in a local heavy rainstorm on the open sea, or as the result of upwelling at the time or previously. Such upwelling may be owing to the turbulence that attends the entry of the Equatorial Current into the Gulf of Mexico (reference 11), to mixture of deeper waters with surface-waters on the turbulent boundary between the Caribbean and Gulf portions of the Gulf Stream, or, perhaps, to more or less localized wind-action at sea. In the last category may be included the deep-stirring action of a hurricane, which has been found to lower surface-temperatures by a degree or more (references 12, 13).

Local cooling of surface-waters and their subsequent drift into the Straits must be taken as a reason for some of the cool streaks, for we find at times within a few miles of the shoals in either winter or summer cool or warm streaks of the same temperature as the cooled or heated shoal waters (Fig. 1).

But those cool streaks, 15 or more miles out in the Straits, could hardly be from drifting shoal-waters. And it is difficult to maintain that local

cooling in the open sea could bring about contrasts of several degrees centigrade in short distances. Upwelling in turbulence at sea is a much more reasonable explanation, especially since the cool streaks so commonly occur between the cooler and warmer portions of the Stream, where, if anywhere, turbulence and mixing to some depth would be expected. Pillsbury's observations of current-rips is in line with this hypothesis (reference 1).

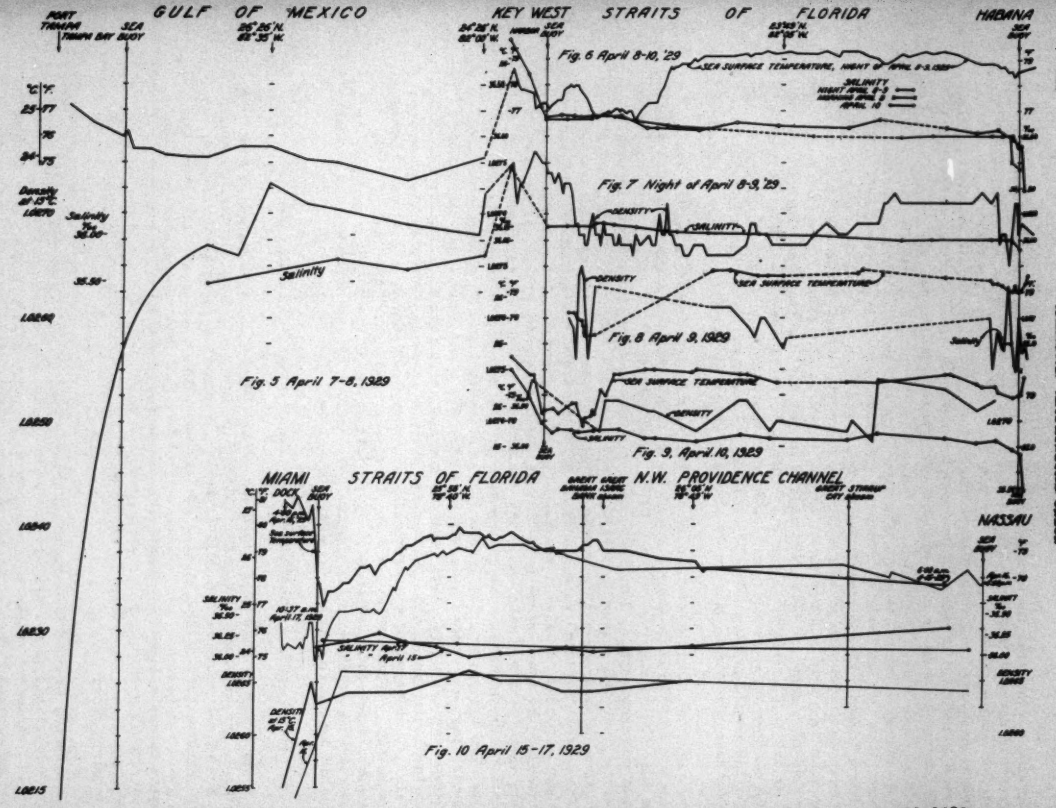
Mixing of currents involving upwelling to the surface cannot normally extend below 150 meters, for otherwise the observed saline layer there could not remain intact. The temperatures of the mixed waters, furthermore, are not low enough for deeper origins, for the very lowest surface-temperatures in cool streaks far from land are about 22° C, which is the general temperature observed at a depth of 50 to 100 meters in the north portion of the Straits and from 150 to 200 meters in the south portion (references 3 and 6). Because of the steeper lapse-rate in the north portion, a certain amount of mixing or upwelling should produce more pronounced cool streaks there than in the south. The thermograms show this to be the case (Figs. 3 and 4).

If upwelling rather than local cooling of surface-waters in the open Straits or Gulf is responsible for the cool streaks the cooler waters should be more saline than the warmer, owing to the presence of the more saline layer at a moderate depth. Therefore, in the absence of an opportunity to sample water at various depths in detail, the writer was encouraged by Dr. Henry B. Bigelow to make observations with a hydrometer and to collect water-samples (for later titration) in conjunction with the temperature-observations the writer expected to make on several crossings of the Straits while checking the automatic and eye-observations made on the Key West to Habana car ferries. This expedition was made under the auspices of the School of Geography of Clark University, with the cooperation of the Bureau of Fisheries, that supplied the hydrometer, the United States Hydrographic Office, and the United States Coast and Geodetic Survey that supplied a map and forms, the United States Weather Bureau, that rendered general assistance at Florida ports, the Peninsular and Occidental and the Florida Inter-Islands Steamship companies, that provided transportation and every assistance on board ship, and the Museum of Comparative Zoology of Harvard University, that titrated the 64 samples of sea-water obtained. In the reduction of the 174 hydrometer-observations (reference 14) and the construction of the profiles, the writer has had the assistance of Miss Edith M. Fitton.

The observed profiles, April 7-17, 1929

The first profile (Fig. 5) began on the Cuba at Port Tampa (2:30 p.m. April 7) and extended to Key West (5:30 a.m. April 8), in order to include waters of the southeastern Gulf of Mexico, a possible contributor of water to the northern part of the Straits of Florida. Outstanding are the rising density and salinity and the falling temperature with increasing distance from inland waters and the coast and with decreasing latitude. The density-observations with hydrometer were made in a pail of water barely deep enough for flotation and are seen to be subject to a systematic error of about 4 in the fourth place.

From Key West to Habana, 10:30 p.m. April 8 to 7:30 a.m. April 9 (Fig. 7), the observations of temperature and density and the samples collected were from the main-condenser intake of the Henry M. Flagler. Chief Engineer C. H. Stanton provided excellent facilities for frequent observations by providing a small outlet-faucet, seat, desk, pail, cloths, thermometer-hook, drop-light, and electric fan in the spacious engine-room. The first four and the sixth



Figs. 5-10--Profiles of sea-water temperatures, densities, and salinities, Florida Straits, April 1929

hydrometer-observation were made in a large pail, the rest in a vase about the size of the vase usually employed for observations, but with irregular sides. Unfortunately, neither the Bureau of Fisheries nor the Weather Bureau could spare a standard vase for these observations. The slight rolling of the ship and the slightly irregular depths to which the vase was filled each time led to contacts at slightly varying angles with the side of the vase, undoubtedly vitiating the results to some extent. The bubbles collecting on the hydrometer were another source of error, making the water appear denser than it really was. Also the elimination of thin films of oil proved difficult. The reduced results when compared with the titrated samples, however, show reasonably close relation, indicating an error of observation of the order of 3 in the fourth decimal place.

Salinities were found to be high on the Florida reefs and close to the shoals, moderately high in the cool water of the north portion of the Straits, and slightly lower in the middle and south portions of the Straits except that near the Cuban shore the salinities were a little above those in the middle. At the harbor entrance, however, the salinity dropped rapidly (Fig. 6).

Further observations were made and samples collected on the return trip to Key West on the Henry M. Flagler in the daytime (Fig. 8) and again on the return trip to Key West in the Joseph R. Parrott 24 hours later (Fig. 9). The salinities are repeated with the profile of the first trip in Figure 6. Temperatures of these two daytime trips are higher than the nighttime ones, owing to the daytime heating by the sun. Captain W. I. Jackson and First Officer R. J. Wall of the Flagler and Captain C. D. Harrington and First Officer R. F. Lord of the Parrott greatly facilited these observations.

The rapid change in salinity and temperature from the open Straits to the reef-water near Key West is shown on the diagram (Fig. 9) and in Table 1. The line between the blue water of the Straits and the light green water of the reefs was passed just inside the Sea Buoy, and was marked by a rise of 0.12 o/oo in salinity and a slight change in temperature. Notable is the high salinity of Key West harbor (36.94 o/oo)--much above those of the Gulf (36.81) or of the open reefs (36.30 to 36.34) and the Straits (36.18). This indicates but slow interchange of water.

A few days later, about the time it would normally take for the water in the Gulf Stream between Key West and Habana at the time of these profiles to reach the latitude of Miami, R. W. Gray of the United States Weather Bureau at Miami and the writer were invited by G. W. Linton, Vice-President and General Manager of the Florida Inter-Islands Steamship Company, to make a round trip to Nassau from Miami on the Princess Montagu for the purpose of observing the Gulf Stream. The trip was made April 15 to 17, amid unusually windy weather and rough seas. While hydrometer-observations were consequently rendered difficult, a series of 190 temperature-observations and 16 samples of sea-water were obtained. An iron bucket was employed. The dips were made on the starboard stern on the eastbound trip, when the ship was traveling with a strong southwest wind. The engine outtakes were on the other side of the vessel, and the waves of considerable size were adequate to mix thoroughly any water coming from the ship on the observing side. On the return trip to Miami, however, the sea was so rough the ship was taking seas over the bow and the starboard side, and the head-wind was too strong to permit observations on this non-outtake side. Rather than have no observations, dips were made on the leeward, port stern. Though the mixing by the heavy sea was considerable, certain temperatures a few tenths of a degree higher than the general run seemed to indicate that some outtake water was occasionally obtained. After the ship was

Table 1--Temperatures and salinities across Florida Straits (samples by C. F. Brooks; titrations by Harvard Museum of Comparative Zoology)

Date	Station	Lat. north	Long. west	Depth	Salinity	Temperature
1929		° '	° '	meters	o/oo	°C
Apr. 7	2001	26 57	82 42	Surface	35.47	24.0
7	2002	26 55	82 21	Surface	35.77	23.9
8	2003	25 20	82 10	Surface	35.64	23.5
8	2004	24 58	82 01	Surface	35.81	24.0 (approx.)
8	2005	24 28	81 48	2	36.17	25.2
8	2006	24 24	81 50	2	36.17	25.5
8	2007	24 20	81 51	2	36.17	24.9
8	2008	24 20	81 52	2	36.20	24.9
9	2009	24 18	81 53	2	36.21	25.3
9	2010	24 17	81 53	2	36.19	25.1
9	2011	24 13	81 55	2	36.15	24.8
9	2012	24 08	81 57	2	36.11	25.8
9	2013	24 06	81 58	2	36.09	25.9
9	2014	24 01	82 00	2	36.07	26.2
9	2015	23 44	82 08	2	36.00	26.2
9	2016	23 29	82 14	2	35.99	26.2
9	2017	23 24	82 16	2	36.00	26.2
9	2018	23 14	82 20	2	36.00	26.0
9	2019	23 11	82 22	2	36.00	25.8
9	2020[a]	23 09	82 21	2	35.89	25.8
9	2021[a]	23 09	82 21	2	35.68	26.1
9	2022	23 10	82 21	2	35.75	26.2
9	2023	23 11	82 20	2	36.00	26.1
9	2024	23 12	82 20	2	36.00	26.2
10	2025[b]	23 09	82 21	Surface	35.47	26.5
10	2026[a]	23 09	82 21	Surface	35.90	26.1
10	2027	23 10	82 21	Surface	35.96	26.1
10	2028	23 13	82 20	Surface	36.06	26.3
10	2029	23 16	82 18	Surface	36.03	26.3
10	2030	23 20	82 16	Surface	36.09	26.6
10	2031	23 32	82 11	Surface	36.16	26.4
10	2032	23 38	82 09	Surface	36.07	26.4
10	2033	23 50	82 04	Surface	36.10	26.4
10	2034	23 55	82 02	Surface	36.14	26.6
10	2035	24 02	81 59	Surface	36.06	26.7
10	2036	24 09	81 56	Surface	36.08	26.7
10	2037	24 11	81 55	Surface	36.08	26.7
10	2038	24 15	81 54	Surface	36.20	26.6
10	2039	24 18	81 52	Surface	36.20	26.2
10	2040	24 19	81 52	Surface	36.19	25.8
10	2041	24 22	81 51	Surface	36.18	25.7
10	2042	24 22	81 50	Surface	36.19	25.9
10	2043	24 23	81 50	Surface	36.20	25.9
10	2044	24 25	81 49	Surface	36.21	25.7
10	2045	24 27	81 49	Surface	36.18	25.8
10	2046	24 27	81 48	Surface	36.30	25.7

Table 1--Continued

Date	Station	Lat. north	Long. west	Depth	Salinity	Temperature
1929		° '	° '	meters	o/oo	°C
Apr. 10	2047[c]	24 29	81 48	Surface	36.64	26.6
10	2048[c]	24 33	81 50	Surface	36.94	26.6
15	2049	25 47	80 05	Surface	36.12	25.5
15	2050	25 49	79 59	Surface	36.20	25.4
15	2051	25 51	79 53	Surface	36.28	25.8
15	2052	25 52	79 48	Surface	36.17	26.2
15	2053	25 54	79 42	Surface	36.11	26.4
15	2054	25.57	79 35	Surface	35.99	26.5
15	2055	25 59	79 29	Surface	36.01	26.4
15	2056	26 02	79 23	Surface	36.05	26.3
15	2057	26 04	79 16	Surface	36.07	26.1
15	2058	26 06	79 11	Surface	36.05	26.2
15	2059	26 05	78 49	Surface	36.10	25.9
16	2060	25 17	77 29	Surface	36.35	25.4
16	2061	25 52	77 55	Surface	36.05	25.8
17	2062	26 02	79 15	Surface	36.06	26.1
17	2063	25 56	79 36	Surface	36.13	26.2
17	2064	25 47	80 03	Surface	36.16	23.8

[a]Entrance, harbor of Habana. [b]Neck, harbor of Habana. [c]Harbor of Key West.

two-fifth across the Straits, however, observations became practicable near the port bow.

The southwesterly gale of April 15, shifting to strong westerly and northwesterly April 16 and 17, produced a notable reduction in sea-temperature in a period of 36 hours. While the lower temperature of the air and increased evaporation were responsible in part, the great stirring of the sea by the wind must have been important. The belt of highest temperature was shifted five or ten miles eastward; the temperatures of the western quarter of the Straits fell 0.5 to 1° C, and Miami harbor temperatures fell nearly 3° C. The waves on April 15 were two to four meters high and with white caps and from the southwest. On April 16 there were westerly white caps near Nassau, and on April 17 in the Straits there was a 4- to 5-meter north ground-swell from an intense northern storm, and 1-to 2-meter white caps on top.

The line between green and blue water was two miles off shore April 15 and three miles out April 17. The blue water April 17 had a dead, muddy-blue color, and floating Gulf weed and fragments of green grass-like weed in it, at a distance of six miles from the shore (temperature 24.2 C), where the water is 200 meters deep. The Miami sea-buoy on the morning of April 17 was leaning southward at an angle of 45° in the strong southbound shore-current.

Salinities on these profiles were much like those between Key West and Habana, higher in general near the Florida side, and highest in the cool streaks and lowest in the warmest part of the Straits. Miami harbor densities were low, owing to the river-water there and Bahamas waters were more saline than the warm part of the Gulf Stream--a condition noted heretofore (reference 2).

Conclusion

Surface-profiles of temperature, density, and salinity across the Straits of Florida show relatively small effects of shore-waters on the conditions in the Straits and suggest upwelling as the chief cause of prominent cool streaks that commonly appear in the Florida third of the Straits.

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SOME REMARKS ON THE BIOLOGICAL PHASES OF OCEANOGRAPHY

Waldo L. Schmitt

So little has been said on the biological side of oceanography that I feel there is some justification for the remarks I am about to make. The present

state of our knowledge of the dynamics, the physics, and the chemistry of the sea, the environment of its plant and animal life, the configuration and constitution of its bottom we owe to the enterprise and the energy of the types of oceanographers represented here yesterday and today.

By the more careful determination of salinity, temperature, hydrogen-ion concentration, silica, nitrogen, and phosphate-content of the ocean, we are approaching a better understanding of the ways of its living inhabitants, and we are furnished thereby with significant indices of the intensity and the magnitude of the life processes that go on in the sea; shorter cuts in estimating the productivity of its waters perhaps than counts and quantitative hauls for specimens.

The factors commonly dealt with are the larger, the more obvious, and more readily ascertainable, and the determinations of these have only approached the desired goal of exactness a comparatively short time ago. But our correlations of the biologic phenomena of the sea with the physical and chemical findings are yet far from satisfying, and an adequate explanation of the succession of communities and populations through the year is still being sought.

For the better understanding of the biological phenomena of the sea it seems that we are in need of more data. Where are these to be found? Perhaps in the traces of things at present overlooked or neglected in our investigations, the rarer elements and salts, things we know or suspect to occur but which we take for granted and of too little account. We are aware that artificial sea-water lacks a necessary something when we undertake to use it as a medium for growing marine organisms. To insure success, it must be activated, as it were, with not more than a small quantity of real sea-water. We know in agricultural practice that boron is necessary for the proper growth of beans and that to crop plants minute traces of manganese, a few parts per million, are as necessary and essential to well being as vitamins are to human beings. Until we know the whole story of each sample of sea-water, we might well be overlooking profoundly important minutiae that are as far-reaching in their effects in the economy of the seas as certain well-known catalysts in modern industrial practice.

The physical and chemical oceanographers are to be congratulated in the great advances they have made in modern oceanography particularly on the dynamic side. But to keep up their end in the proper correlation of the biological phenomena with the more complete determination desired of the make-up of the sea, the biologists must progress considerably beyond the present dictionary method of listing names of animals. Beginnings have been made, but there is still a considerable lag in biological oceanography as compared with physical oceanography. This may be due to a variety of causes. The chief of these seems to be the critical dearth of taxonomic specialists; biologists who have a well-grounded knowledge of the various groups of marine organisms from the lowest forms of life up to and including, strange as it may sound, the fishes.

I wish to take decided exception to some of the fairly recent reports of congresses and committees on oceanography giving lists of specialists on this and that group of marine organisms. Either the sad state into which systematic marine biology has fallen is not appreciated, or else the true facts are being glossed over as is often done in the case of bad habits and poor relations.

Where can one get marine protozoa, except foraminifera, determined today, or sponges, coelenterates--hydroids, actinians, alcyonarians, corals, and the

like? The various groups of marine worms all go begging, and though it is still possible to get an occasional nemertean or annelid named, getting larger or more comprehensive collections worked up is simply out of the question. This is also virtually true of even the higher crustacea; and the lower or so-called entomostracan forms other than copepods must go unstudied. So it goes with nearly all groups of invertebrates, the lower chordates, and even the fishes. The active trained systematists interested in fishes, in spite of all the fisheries research that is being done, can be counted on the fingers of one hand. Every taxonomic worker today has on hand at this very moment much more material awaiting determination than he will be able to cope with in his lifetime. The situation as regards phytoplankton is no better.

A recent questionnaire on Pacific material reveals that in the National Museum there are more than 700,000 marine invertebrates, fully two-thirds of which are still undetermined. And this does not include the myriads of microscopic organisms in some thousands of bottom-samples.

Dr. Bigelow's Gulf of Maine report is a remarkable and monumental work, but unworked and unstudied in our Museum storage are hundreds of plankton-samples taken back in the eighties. Coming from the same general region, these should afford a most interesting basis for comparison of present conditions with those obtaining almost half a century ago.

One can readily learn to distinguish between the commoner animals of a region, but without the guidance of a trained hand, many a serious error is apt to creep in; finer and more exact distinctions are often needed, and proper systematic training is not acquired overnight. I could cite for you quite a number of unfortunate mistakes, often ludicrous, but ever regrettable, that were the results of neglecting the taxonomic side of the animals and plants that have been worked upon by this or that ecologist, physiologist, or experimental biologist, and others.

One of the most promising of oceanographic expeditions in view of the intimate correlation of results, biological and physical, that is to be expected has been the last cruise of the Carnegie. But it surely will not be an easy matter to get the biological materials worked up as rapidly as the physical and chemical data in order to obtain the greatest return from correlated observations. It is the dearth of trained taxonomic workers that will be the greatest obstacle to be overcome in reducing the biologic data of this Expedition.

More reporting upon and listing of the collections of the best known of the Albatross cruises, those under the Agassiz auspices, and all prior to 1905, is still far from complete.

Conditions in this direction will grow worse instead of better. Each year records the loss of one or another of the outstanding taxonomic authorities, without the appearance of another or younger man to take his place. Opportunities must be made and positions provided for more systematic workers, for soon there will be an end of those now living. Just recently a very excellent piece of work was done on an important marine crustacean life history, habits, etc. This is the kind of marine biologic work that is much needed and which should be encouraged and continued, but the man who did it is unable to find a position. Perhaps the paper was too systematic in its identification of the various larval stages of a complicated life history.

In view of the value, the necessity of knowing our animal life of the sea,

and the decreasing group of taxonomic workers each year, it is not too much to expect and ask that each of the phyla of invertebrate marine animals, grouping the lower forms of worms as one, be endowed with something like a million dollars each. The income would no more than suffice to keep a small staff of the most necessary specialists going and provide for the needed recruits and understudies, help, and facilities. One cannot create a supply of specialists on mere demand alone. The need for them is ever with us but not the positions for them to fill. Some such endowment will have to be provided sooner or later.

Besides the mere naming and listing of the marine organisms of any expedition more far-reaching things need to be accomplished by the biologists. The physiological constants of the species of marine animals, as they have been called, need to be determined for all possible marine organisms, if proper headway is to be made in biological oceanography. These constants, as suggested by Dr. Orton of the Marine Biological Association, Plymouth, England, should include: Temperatures recorded under normal salinity-conditions of the (1) upper death-limit, (2) upper limit of growth or hibernation conditions, (3) maximum limit for breeding, (4) minimum for breeding, (5) lower limit of growth or hibernation conditions, (6) lower death-point, (7) rate of change of temperature at different temperature-levels causing death, and (8) the variations in these constants in different areas, and in different varieties and sub-varieties of the animals in each area.

If sufficient information were available, one would be able to plot the limits of the geographical distribution of species, of course predicated on the data obtained by the physical oceanographers. On the other hand, determined collections of specimens and their correlation with physical data secured by oceanographic expeditions would likewise throw light on many of the physiological constants (of species).

From the vegetation or the weeds on a tract of ground, we can tell a great deal about the condition and kind of soil on which they grow. So, too, may we be able to tell more about the conditions obtaining in the sea at different times, places, and depths by means of its organic life there supported. The lag of biological oceanography behind the physical cannot but act to the detriment of the latter in the long run.

Another of the crying needs of all phases of oceanography is the reduction of data. Dr. Vaughan has stressed this on a number of occasions. It applies to biological as well as physical data. As we have standard nets, apparatus, and methods of analysis, a standard system of record-keeping should likewise be adopted, with a view to the application of modern census methods. With a proper punched card system, it should be possible to mechanize the assorting and correlation of much, if not all, data. With the aid of card-sorting machines like those used by the Census Bureau, it would be possible to release the wealth of biological and zoogeographic information buried in the systematic catalogs and card-files of museums, oceanographic, and kindred institutions, and by such means to escape being overwhelmed by the ever-rising tide of oceanographic data.

U. S. National Museum,
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TIDAL WORK OF THE COAST AND GEODETIC SURVEY

Paul C. Whitney

The tide-work of the Coast and Geodetic Survey arose in connection with the primary purpose for which the Bureau was established, namely, the charting of the coast. Because of the rise and fall of the tide, the depth of water at any point is constantly changing. Therefore, to properly chart the depths on hydrographic charts some uniform level or fixed datum-plane must be adopted.

From its inception as a detail in connection with the charting of the coast, the tide-work has expanded until it has become recognized as one of the major activities of the Coast and Geodetic Survey. This expansion has been an organic growth made necessary by the expanding of the activities of the Survey. The various phases now covered by the tide-work may be summarized under the following heads: (1) Determination of datum-planes; (2)establishment of tidal bench-marks; (3) tide- and current-surveys of harbors; (4) publication of tide- and current-predictions; (5) maintenance of authoritative records of rise and fall of the tide, and flow and ebb of the current; and (6) study of tide- and current-phenomena. At the present time I desire to discuss in a few words each of these phases of tide-work and their relations to oceanography.

(1) Datum-planes--Heights on land as well as depths in the sea must be reckoned from some fixed level which is adopted as the plane of reference. The basic datum both for heights on land and depths in the sea is that of mean sea-level. For many purposes the level of the sea at any moment may be taken as an approximation to mean sea-level. But for geophysical purposes this obviously is insufficient. The accurate determination of mean sea-level at any point is a problem which the other sciences look to oceanography for its solution.

Now it is the tide that is primarily responsible for the varying level of the sea. And since the period of the tide is approximately half a day, it would appear at first thought that by averaging the varying heights of sea-level for half a day or perhaps a day a good determination of mean sea-level should result. As a matter of fact, however, as is well known, sea-level varies from day to day, from month to month, and from year to year. The accurate determination of mean sea-level is therefore a problem of considerable magnitude and complexity. In fact, a primary determination of mean sea-level at any point, that makes any pretense at precision at all, requires a number of years of observations.

As a technical problem in connection with its engineering operations the Coast and Geodetic Survey has developed methods by means of which a sufficiently accurate determination of mean sea-level can be derived from a year of observations or even less. This involves the comparison of simultaneous observations at the point for which mean sea-level is desired and some primary tide-station at which long-continued observations have been secured. For this purpose about 25 primary tide-stations are maintained continuously in operation on the Atlantic, Gulf, and Pacific coasts of the United States, and in Alaskan waters.

In various fields of work, other datum-planes are necessary, such as the datums of mean low-water, lower low-water, mean high-water, and higher high-water. These are even more difficult to determine with precision than mean sea-level is, since they are subject to the same variations as sea-level and others besides. However, methods have been developed for determining, by comparison with the observations at a primary tide-station, the various tidal

datum-planes from short series of observations with a very considerable degree of precision.

(2) Establishment of bench-marks--To preserve the datum-planes determined at any point for the future, and to make these readily accessible for all purposes, bench-marks are established in connection with the tidal observations made by the Coast and Geodetic Survey. A number of bench-marks are established in rock or concrete, or on substantial buildings at all places where tide-observations are made. And in connection with these, tidal bench-mark publications are issued which list the bench-marks in the various localities, giving descriptions which serve to identify them easily and giving also the heights of these bench-marks with regard to the various tidal datum-planes. At the present time tidal bench-mark publications have been issued for six states on the Atlantic coast and for two on the Pacific coast. It is planned to cover the other coastal states as rapidly as possible and to keep these bench-marks up to date by periodic inspection and leveling.

These bench-marks, therefore, furnish points of reference along the coast from which mean sea-level, mean high-water, mean low-water, and various other datums can be readily derived. This means that the oceanographer engaged on some problem which requires as a basis the determination of one or other of these datums, may begin his work with that part of it already solved.

At a number of the primary tide-stations where observations covering many years have been secured there are being established permanent concrete monuments, known as basic bench-marks. With foundations extending deep into the ground, they are designed to maintain their elevations over a long period of time and provide the engineer with standard tidal datums determined from long series of observations. In the future they will doubtless play an important part in deciding the much-discussed question of the elevation or subsidence of our coasts. Nine of these basic bench-marks have already been installed, six on the Atlantic coast and three on the Pacific coast of the United States.

(3) Tide- and current-surveys of harbors--The present program of work contemplates a comprehensive tide- and current-survey of some important harbor of the United States each year, as a part of the tidal work of the Coast and Geodetic Survey. This has for its purpose the determination of the characteristics of the tides and currents in the particular harbor in question. This involves tide-observations at various places and current-observations at various points and at various depths. The results are issued in a publication devoted to the particular harbor as soon as the data have been tabulated and correlated.

As a result of this work we are becoming familiar not only with the tide- and current-conditions in the various harbors, which is the primary purpose of these surveys, but at the same time we are gaining a better understanding of the general tide- and current-phenomena in coastal waters. That phase of oceanography is being very materially expanded as a result of these harbor-surveys.

In studying the data derived from these harbor-surveys, every effort is made to correlate the results derived from the present observations with the results of primary observations. This procedure brings out any changes that have resulted in the tide- and current-features. At the present time such comprehensive surveys have been made for the following water-ways: New York Harbor, San Francisco Bay, Delaware Bay and River, Southeastern Alaska, Boston Harbor, Portsmouth Harbor, Chesapeake Bay and Tributaries, Long Island Sound

and Hudson River. It is planned to continue these surveys until all the important harbors and coastal waterways of the country are covered.

(4) Publication of tide- and current-predictions--Each year the Coast and Geodetic Survey publishes tide- and current-tables to cover the following calendar year. These tables give predicted times and heights of the tide and the predicted times and velocities of the current in a number of important harbors. The tide-tables cover the whole maritime world, the 1930 issue of these tables giving the daily predictions for 89 of the more important parts of the world. In addition, there are given tidal constants and differences for some 3,500 other places; so that by the use of these tide-tables the time and height of the tide can be predicted with a considerable degree of precision for practically any point along the coasts of the world.

The current-tables cover primarily the coasts of the United States. Daily predictions are given for 13 important points on the Atlantic coast, ten on the Pacific coast, and one in the Philippine Islands. In addition, there are given current-constants for more than 1,000 other places by means of which daily predictions for these places can be readily derived.

In connection with the prediction of tides and currents for the tide- and current-tables, harmonic analyses are required. As is well known, the harmonic constants furnish the fullest information for the tides in any given region. So that while these harmonic analyses are made primarily for the technical purposes of the tide- and current-tables, they furnish at the same time extremely valuable data to oceanography.

(5) Maintenance of authoritative records of tides and currents--Since the Coast and Geodetic Survey is the only agency in the United States that deals in any comprehensive way with the tide, the duty of maintaining files of authoritative tide- and current-data for use of the Government and the public generally devolves upon it. These data are derived from the records of the primary tide-stations, from the results of the tide- and current-surveys of the harbors, from tide-observations made in connection with hydrographic surveys and from the predictions in the tide- and current-tables. From these records it is possible to secure, for the not too distant past, authoritative data on the state of the tide and current at many places.

The very great cost of securing current-observations and the lack of thoroughly satisfactory automatic current-meters, make the current-part of this phase of the work lag very much behind the tidal part. However, it is hoped in time to develop automatic current-meters which will enable automatic current-records to be secured in the same way that automatic tide-records are now secured. When that time comes it will be possible to secure records of the ebb and flow of the current at various places.

(6) Study of tide- and current-phenomena--In order to carry out the technical duties which devolve upon the Coast and Geodetic Survey in connection with its tide- and current-work, it is found necessary to make a study of tide- and current-phenomena in general. As in other fields of work, it is impossible to draw a line between the technical and the scientific phases of tide- and current-work. Starting with the practical need of correcting soundings for purposes of chart-making, it became necessary to study the whole subject of tides and tidal phenomena in order that the practical purposes which were to be served might be carried out efficiently. In connection with this, work of research character has been and is still being carried on in regard to the various phases of the

work on tides and currents enumerated above and also in connection with improvement on existing instruments.

U. S. Coast and Geodetic Survey,
Washington, D. C.

ELECTRICAL CONDUCTIVITY-METHOD FOR DETERMINATION OF SALINITY ABOARD SHIP

F. W. Wenner, E. H. Smith, and F. M. Soule

(Abstract)

At the request of Dr. S. W. Stratton, acting as a member of the Interdepartmental Board on International Service of Ice Observation, Ice Patrol, and Ocean Derelict Destruction, the apparatus described was designed by Wenner at the United States Bureau of Standards in 1922. The instrument was intended for the measurement of the salinity of sea-water quickly and reliably under the severe conditions existing on board the cutters of the United States Coast Guard on ice-patrol service. The electrical conductivity-method was employed and the device met the requirement of having a precision of about 0.01 o/oo salinity.

The instrument is essentially a shielded Wheatstone bridge using alternating current of 1,000-cycle frequency and having a Wagner branch for bringing the telephone-receivers to ground potential. Two adjacent arms of the bridge are formed mainly by similar electrolytic cells which are immersed in a stirred and thermostatically controlled water-bath. The other two arms of the bridge are formed by two equal fixed resistances between which is a slide-wire having 1,000 divisions. The resistance of the slide-wire is 1/15th total resistance of these two arms. A substitution method is used, thus making the measurement practically independent of the cell-constants.

It can be assumed that over the range of salinity usually encountered in sea-water the relation between electrical conductivity and salinity is linear and can be expressed in the form

$$C = C'(A + BS) \qquad (1)$$

where C is the conductivity at any salinity S and C' is the conductivity at some other salinity S', and A and B are constants depending upon C'.

If s' is the slide-wire reading corresponding to a salinity S' when the bridge is balanced, m is the resistance of one division of the slide-wire divided by the resistance of the arm including part of the slide-wire and adjacent to the measuring cell, n is the resistance of one division of the slide-wire divided by the resistance of the arm including part of the slide-wire and adjacent to the auxiliary cell, and s is the slide-wire reading corresponding to any salinity S, then it can be shown that the calibration-curve for such a bridge can be expressed by the equation

$$S = S'\left[1 + \frac{(n + m)}{s'B}(s - s') + \frac{(mn + m^2)}{s'B}(s - s')^2 + \frac{(m^2n + m^3)}{s'B}(s - s')^3 \ldots\right] \qquad (2)$$

where terms involving $(s - s')$ to exponents greater than three may be neglected. From the experimental work of E. Ruppin (see O. Krümmel, Handbuch der Oceanographie, Bd. I, 291) and A. L. Thuras (see J. Acad. Sci., Washington, D.C., v. 8, 685, 1918) on the constants in equation (1) we can predict with reasonable assurance that a fair approximation to the calibration-curve of such an instrument

will be given by the equation

$$S = 35\left[1 + \frac{(m+n)}{0.89075}(s - s') + \frac{(mn + m^2)}{0.89075}(s - s')^2 + \frac{(m^2n + m^3)}{0.89075}(s\ s')^3 \ldots\right] \quad (3)$$

in which s' has become the slide-wire reading corresponding to a salinity of 35.00 o/oo and n and m depend on the selection of s'.

Three bridges have been built and used--two on Ice Patrol Service mainly by Smith and one on the ill-fated non-magnetic vessel Carnegie by Soule. Smith also used one of the first two on the Marion Expedition. The experimentally determined calibration-curves of these instruments were in good agreement with equation (3)

A special feature of these bridges is the use of two similar cells in adjacent arms. When both cells are filled with sea-water, even though the salinity of the sea-water in one is not the same as that in the other, both have very nearly the same temperature-coefficient. As a consequence, the balance of the bridge is not greatly affected by small uncertainties of the temperature, provided both cells are very accurately at the same temperature. As in all bridge-measurements, the precision obtainable is limited among other factors by the heating produced by the test-current. However, with this arrangement the test-current raises the temperature of the sea-water in both cells above that of the bath, in which the cells are located, by substantially the same amount since the thermal time-constants of the two cells are closely alike. Consequently a much larger test-current may be used than would otherwise be permissible. Further, the complicated action taking place at the electrodes, generally referred to an electrolytic polarization, will be substantially the same in both cells, so that the effect of polarization in one cell upon the balance of the bridge is largely neutralized by the effect of polarization in the other cell.

Another special feature not really a part of the bridges but a part of the plan for their use and kept in mind during the design is that a supply of Copenhagen standard sea-water or sea-water of known salinity or known density is carried aboard and used in periodic standardization of the equipment. Samples of the sea-water to be tested, and samples of standard sea-water or sea-water whose properties have been determined by laboratory methods, are substituted one after the other in the same cell. This gives a direct comparison between the unknown and standard samples almost if not quite independent of the cell-constant, largely independent of other constants of the equipment, and largely independent of systematic errors.

The experience obtained in the use of these instruments has demonstrated that they are well adapted to the measurement of salinity aboard ship and the results obtained with them when used in accordance with the recommended procedure indicate that measurements of salinity can be duplicated to within 0.02 o/oo. With certain minor changes which it is recommended be incorporated in future instruments the performance can be expected to be somewhat better.

The complete paper with illustrations will be published either in the August or September 1930 number of the Bureau of Standards Journal of Research.

U. S. Bureau of Standards (F.W.W.), U. S. Coast Guard (E.H.S.), and
Dept. of Terrestrial Magnetism, Carnegie Institution of Washington (F.M.S.),
Washington, D. C.

SECTION OF VOLCANOLOGY

(Washington, D. C., May 1, 1930)

CENTRAL AFRICAN VOLCANOES IN 1929

N. L. Bowen

The existence in Central Africa of a group of volcanoes, some of which are active, has long been known. They lie in the Central African Rift Valley (the western branch of the Rift Valley as distinct from the Eastern or Great Rift Valley) in the region where Uganda, Belgian Congo, and former German East Africa come together. There are eight major volcanic piles named from east to west, Muhavura, Mgahinga, Sabinyo, Wissoke, Karissimbi, Mikeno, Namlagira, and Ninagongo, with a large number of other vents that are topographically less conspicuous (see Fig. 4). The highest is Karissimbi (14,865 feet) and the lowest Namlagira (10,046 feet). The distance from east to west (Muhavura to Namlagira) is 33 miles. The area in which the volcanoes lie, whatever the true mechanics of its formation may be, has the appearance of a sunk-land which is a lateral expansion of the Western Rift. (A. D. Combe in Ann. Rept. Geol. Survey Uganda for 1928, p. 14, calls this expansion the Bufumbira Bay, a very appropriate name. The mountains are usually known in German literature as the Virunga volcanoes, in English literature as the Mufumbira volcanoes, or again they may be referred to in either as the Lake Kivu volcanic area on account of their proximity to Lake Kivu.)

Only two of the volcanoes, Namlagira and Ninagongo, have been active in historic times. They lie at the westernmost extension of the range in the Belgian Congo. The volcanic area was visited in September to October 1929 by a party consisting of Mr. A. D. Combe of the Uganda Geological Survey and of Prof. Bailey Willis and the writer of the Carnegie Institution of Washington. The equipment of the expedition was made available by the government of Uganda. Not only that, but Mr. Combe, who had already made a detailed study of the part of the volcanic area lying in British territory, was enabled to go with us as our guide and mentor. For these courtesies the other members of the party are deeply grateful to Mr. Combe and to the director of the Geological Survey of Uganda, Mr. E. J. Wayland. The present paper is intended to be merely a brief notice of the condition of the volcanoes at the time of our visit.

Ninagongo and Namlagira were in an active phase. As with all volcanoes, their activity is intermittent and Mr. Phillips, District Commissioner at Kabale, Uganda, has called my attention to an apparent periodicity in the case of these two volcanoes with maxima about every eight to nine years. He cited activity in 1897, in 1905, in 1912-13, in 1920, and in 1928-29. But there are records of strong activity in Namlagira in 1894, in 1896, in 1899, and in 1902, with no apparent reason for singling out 1897 as the maximum of this long period of nearly, if not quite, continuous activity. Nevertheless there is a suggestion of some regularity of cycle. The period seems to be definitely shorter than the 11-year sunspot-period with which variation in the level of the African Great Lakes coincides (C. E. P. Brooks, Geophysical Memoirs No. 20, Meteorological Office, Air Ministry, 1923). Only continuous observation, which has become possible of recent years through the residence of Europeans in the vicinity, will supply accurate information as to the periodicity, and only when that information shall become available with the passage of time will there be an adequate basis for speculation as to the cause of the periodicity.

Upon our approach to the volcanic area, Ninagongo (Mother of Gongo, a mythical native hero) was observed to be "smoking." Vapors rose from all parts

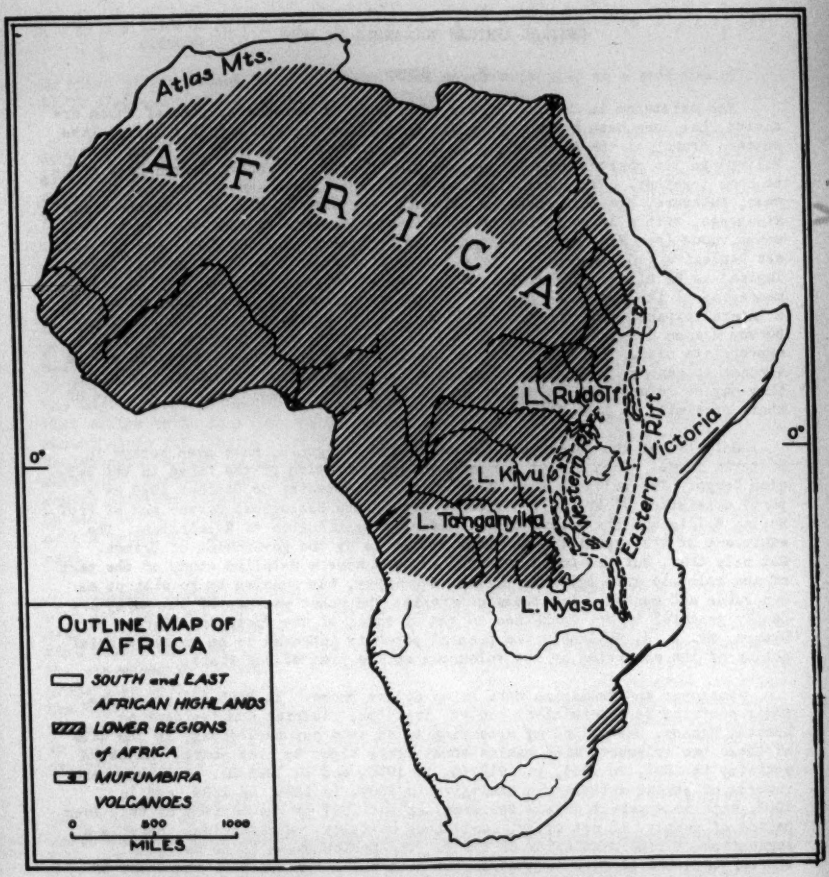

Fig. 1--Outline map of Africa showing location of Mufumbira volcanic area, its relation to African Highlands and the Rifts (necessarily generalized) which traverse these highlands (based on Krenkel's maps)

of the crater which was apparently filled with them. It seemed, therefore, that conditions of visibility in the crater would be such as to render profitless a climb to the summit. Namlagira (The Commander), on the other hand, was giving off only moderate amounts of condensable vapors, enough to form a transient cloud above it. From this cloud a red glow was reflected which was visible at night, a condition indicating the existence of live lava in the crater.

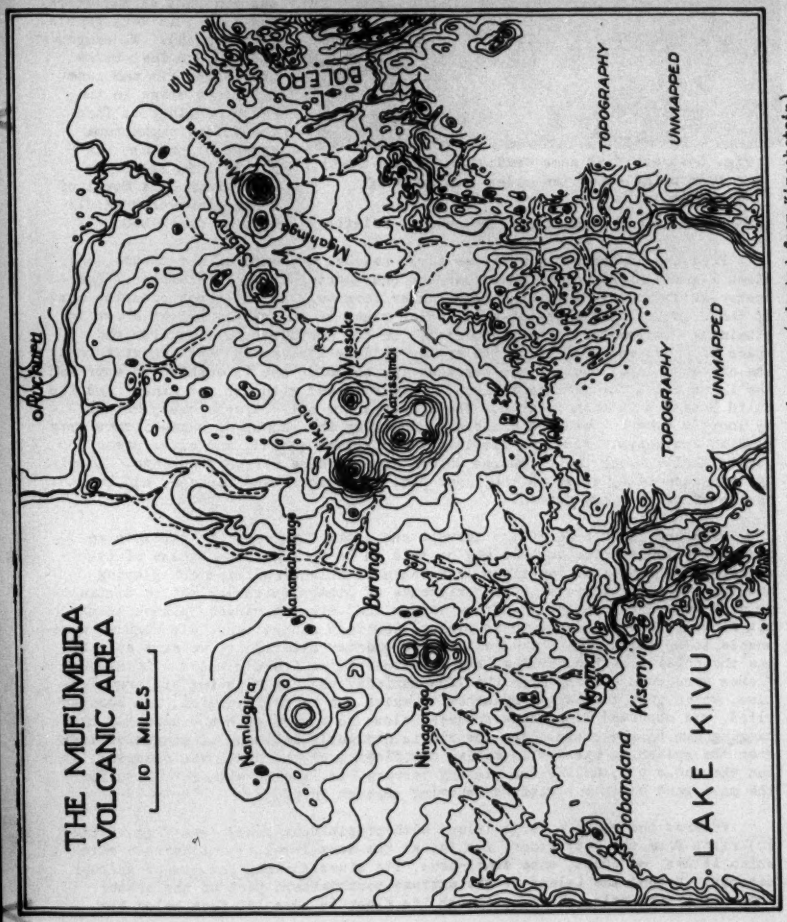

Fig. 2--Map of Mufumbira volcanic area (adapted from Kirschstein)

Fig. 5--Great flat cone Mamlagira (10,046 feet) from point about ten miles southeast of summit

Mamlagira was accordingly ascended. Our journey took us across the fairly rough lava-field made by the 1906 flow from Kanemaharagi, an inconspicuous parasitic cone at the eastern foot of Mamlagira. The ascent is very gentle (see Fig. 5). We camped about 2,000 feet below the crater rim and made this last stage in the early morning, to find the volcanic panorama spread before us. In Figure 6 is shown the photograph of a model of the crater which should help the reader to understand the general relations now to be described.

From the northern rim where we first reached the edge of the crater, one views a great amphitheater about one and one-quarter miles in diameter. The crater-wall is a cliff falling to an inner terrace. This terrace occupies most of the area inside the crater-wall, which rises to a variable height above it, attaining a maximum of perhaps 150 feet. On the northwestern side, for the space of about one-half mile, the crater-wall is lacking and one can walk into the crater on the level of the terrace. By so doing one is enabled to approach the inner pit. The terrace is found to be surfaced with ash and cinder and its solid basement is much cracked. The fissures are often treacherously bridged by loosely coherent ash and cinders, much after the manner of snow-bridges over glacial crevasses. From the fissures steam issues quietly and a few green plants thrive along their courses across an otherwise barren landscape. Locally, and especially close to the inner pit, sulphurous gases escape, and deposits of sulphur are formed.

From the inner edge of the terrace one looks down into the pit upon an impressive scene. The volcano may be said to be in a Kilauean phase of its activity. There is at least one lava-basin in which fountains are playing and there are indications of the existence of others which can not be distinctly made out on account of the dense vapors that rise at various places in the pit floor. The visible basin is perhaps 150 feet across, and, although we were unable to approach closer than within one-quarter mile of it, we were able to see the details of the boiling lava with the aid of field glasses. Occasional flames were seen playing over the lava-surface, and the bursting of large bubbles, which give the larger splashes to which the term "fountains" has been applied, was observed to be more frequent close to and beneath the slightly overhanging rim of the basin. The low rim is obviously built up of congealed lava from the splashing basin. No actual overflowing of the basin was observed, but the whole pit, unlike the cindery terrace, is floored with ropy lava for the most part black but with red showing through in places.

Towards the south a large island with precipitous walls (see Figs. 9 and 10) rises from the ropy floor, its top at the same level as the terrace with which it was, no doubt, once continuous, its sides showing patches of sulphur deposit. Beyond the island at the extreme southwestern part of the crater there is a sink-hole, which, although its floor is some 150 feet below the

Fig. 3

Fig. 4

Fig. 3--Muhavura (13,547 feet) and Mgahinga (11,400 feet) at eastern end of range--Mgahinga still has a well-preserved crater with crater-lake. Fig. 4--Some of the many cinder-cones and minor vents north of main range

Fig. 6--Photograph of model of summit of Namlagira as viewed from southeast (W, main outer crater-wall; T, inner terrace; P, lava-pit; F, fountain-basin; I, island; S, sink-hole)

Fig. 7

Fig. 8

Fig. 9

Fig. 10

Fig. 7--From north rim of crater of Namlagira--in foreground the terrace with a line of solfataras and in background the southern wall of crater. Fig. 8--Edge of terrace and steaming lava-pit of Namlagira. Fig. 9--Crater of Namlagira from point on terrace near southern crater-wall; in foreground, terrace dropping off into sink-hole;in middleground, the island rising from floor of lava-pit and the straight cliff-edge forming northern rim of sink-hole; background, to left of island a promontory of terrace rising to same level as top of island and in extreme background main northern wall of crater. Fig. 10--Part of crater of Namlagira showing cliff which is southern boundary of lava-pit and at same time northern wall of sink-hole, with great black cones piled against its base where lava has spilled from lava-pit into sink-hole; the source of lava is in the fountain-basin which lies in background where maximum steaming is seen.

level of the lava-pit just described, is itself devoid of activity of any kind. (Evidently this sink was not present in 1907, for it is not mentioned by Kirschstein.) Along the cliff which forms the northern wall of this sink and therefore the southern edge of the lava-pit an interesting phenomenon was seen. At various points a few feet below the edge of the cliff lava issued and fell into the sink-hole. This was occurring at four points along the cliff at the time of our visit. The lava at times took the form of a red stream which poured freely as from a spout and fell sheer to the floor of the pit, collecting there as a pool, or again it formed a succession of great drops which were colder and acquired a dark surface as they fell, alighting with a dull thud as might a lump of warm pitch. These piled up against the base of the cliff (see Fig. 10). At many other points along the cliff there had been similar streaming quite recently, as evidenced by the streaking of the face of the cliff by many streams now congealed and black. The points of issue of the lava no doubt change frequently. The actual volume of lava that has thus "leaked" into the sink-hole is quite small. This lava in all probability comes from the main source of activity in the basin with its playing fountains, and flows for a distance of about one-half mile through lava-tubes that are at most only a few feet below the floor of the lava-pit and in places are sufficiently close to the surface to show red, whence the red streaks at various places on the pit-floor. The primary source of magma in the crater is thus strongly localized and lies at the position of the lava-basin. No better evidence of this localization could be desired than the fact that the sink-hole with a floor some 150 feet lower than the lava-pit is, as we have stated, devoid of activity of any kind. In addition to the driblets of lava from the northern wall of the sink-hole, a rather considerable stream of lava, or more probable a succession of streams has evidently quite recently passed around the eastern edge of the island and poured into the sink from its eastern side. It is now cold and its surface lies at a steep angle of perhaps 45°, destroying the cliff aspect of the eastern edge of the sink.

That the past activity of Namlagira has been principally a quiet pouring out of lavas sufficient to build up its great, flat cone is clear from the kind of material one passes over in approaching the summit. Nevertheless, it is evident, too, that explosive phases of activity have not been lacking. (The throwing out of lapilli and of glowing cinders was observed in 1907. Kirschstein, Wiss. Ergeb. d. Deutsch. Zentral Afrika Exped. 1907-8, Band I, pp. 102-110.) The bare upper slopes are strewn with bombs thrown out from the crater. These are sometimes of lava but are often of coarsely crystalline varieties of rock showing a general consanguinity with the lavas and offering interesting evidence as to their plutonic relatives. Again there are bombs, and inclusions in bombs, of wholly foreign rocks such as gneiss and quartzite of the basement rocks. These must be regarded as affording the best evidence as to the kind of materials available to the magma when the question is raised of the origin of the magma through the absorption of foreign rocks. These questions may be considered further when the materials collected have received more extensive study.

The crater-walls of Namlagira are of bedded flows perhaps ten feet thick but with a strong jointing that gives blocks about a foot thick, the whole simulating a wall of masonry. The walls were probably established by the foundering of the central area and since their establishment there has been no complete filling of the crater basin with lava, with overflowing of the rim. There are, however, beautifully fresh lava-cascades only a few hundred feet below the crater-rim. These must have been formed as a result of the lava finding a vent laterally through a fissure in the wall.

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VOLCANIC ACTIVITY IN THE BERING SEA

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(Abstract)

The area of Bering Sea is one of comparatively recent subsidence, the western portion being deep and the eastern shallow. Along its southern border is the Aleutian Arc, a line of islands, mostly volcanic, with many active volcanoes, including Bogoslof, St. Augustine, Shishaldin, Makushin, Katmai, and others. The present note is to call the attention of the Section of Volcanology to the apparently little-known fact that there has been very recent, if not present, volcanic activity north of the Aleutian Arc, within the eastern portion of Bering Sea.

Submarine eruptions are recorded by Landgrebe in 1855, and are noted by Sapper and by von Wolff, to have taken place in 1815, east of St. George Island, one of the Pribilof Islands. Remains of these are to be seen in several small, circular shoals marked on the charts east of St. George, with depths of five to eight fathoms.

In a book that is scarcely known now to geologists H.W. Elliott (Our Arctic Province, New York, 1887, p. 461) states that Pinnacle Islet, about 1,000 feet high, and situated about five miles south of St. Matthew Island, north of the Pribilofs, was in a state of eruption, with "volcanic fire and puffs of black smoke" near the summit at the time of his visit to St. Matthew. He does not mention lava-flows, but says that the volcano was thus active at the time of its discovery and has been so active ever since. The activity of this volcanic island is not recorded in the earlier volcanic catalogs of Daubeny, Landgrebe, Scrope, or Fuchs, nor in the later ones of Mercalli, Sapper, and von Wolff.

It may be mentioned that recent studies by me show the presence of nephelite basanite on St. Paul, one of the Pribilof Islands, and that some descriptions by Elliott indicate the possible presence of trachytic rocks on St. George. The volcanic rocks of the inner Bering Sea area would, thus, resemble those of the main inner Pacific Basin, and differ widely from those of the Aleutian Arc.

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THE GENERAL CHARACTER OF DEEP-SEATED MATERIALS IN RELATION TO VOLCANIC ACTIVITY

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It is unlikely that a satisfactory explanation of volcanism will be obtained without having some sort of picture of what lies at considerable depths below the surface of the Earth. The underlying causes of volcanic activity are to be found in the physical and chemical nature of deep-seated rocks. The object of this note is to summarize briefly the present state of our knowledge concerning the materials of the Earth below the relatively thin film accessible to direct observation. Unfortunately, not much is yet known--the outlines of our picture are being slowly filled in but it is still only a sketch. For supplying the details volcanology will lean heavily on the kindred science of seismology, and recent developments in pure physics give promise of important aid.

Origin of the Earth--It is quite impossible to form any notion of the interior of the Earth without first deciding upon the manner of its formation. As in the case of living organisms, its origin and juvenile behavior are the principal factors in determining the characteristics of the adult Earth. There is now general agreement that the planets, including our own, were originally formed through tidal disruption of the parent Sun by the close approach of another star. But although the method of its original formation, as advanced by Chamberlin and Moulton (T. C. Chamberlin, Astroph. J., v. 14, 17, 1901; F. R. Moulton, ibid, v. 22, 165, 1905) and others (J. H. Jeans, Problems of cosmogony and stellar dynamics, Cambr. Univ. Press, 1919; H. Jeffreys, The Earth, Cambr. Univ. Press, 2nd ed., 1929), is universally accepted, there is no such unanimity of opinion concerning the subsequent development. Two different views are maintained by two opposing schools--according to one the hot gas pulled out from the Sun cooled first to a liquid and then to a solid globe, while according to the other the material cooled first to small solid pieces (planetesimals) whereupon the planetesimals fell into the central nucleus forming the present Earth by comparatively slow accretion. To review all the arguments of these two schools would be entirely beyond the scope of this note, but it seems to the writer that anyone approaching this subject for the first time, and without previous bias, would find that the notion of a molten globe which has cooled to a solid crystalline body offers so easy and natural an explanation of the known facts of geophysics and presents so few major difficulties as to leave in his mind very little doubt of its basic truth. Probably the issue should be decided through the mathematical analysis of the behavior of a hot gaseous body of the mass of the Earth; but apart from these considerations a decision might well be reached on the basis of what is now known about the Earth's outer shells. That the outer layer (in continental regions) consists almost entirely of granite is demanded by the facts of seismology, the physical properties of rocks, and gravity-measurements in continental and oceanic regions, while almost equally cogent reasons indicate that below 50 to 60 km there is present for a depth of thousands of kilometers only peridotite rock. So sharp a segregation follows simply and directly from the notion of an originally molten Earth, but only with awkward difficulties if it were initially solid.

In what follows there is given for deep-seated materials, a set of properties which are mutually consistent and which agree with the idea of an Earth which was once entirely liquid. This liquid globe cooled rapidly by radiation from the surface and convection throughout the mass until at the bottom of the silicate part the freezing temperature, under the pressure at that depth, was reached. (The formation of an iron core is not pertinent to the present discussion.) Subsequently, crystallization, from the bottom upward (L.H. Adams, J. Wash. Acad. Sc., v.14, 467, 1924), proceeded regularly and rapidly, the remaining liquid being stirred by convection and losing heat rapidly by radiation from the hot surface. The material which crystallized out--principally olivine--formed a steadily increasing thickness of peridotite, while in the superincumbent

liquid layer there was steadily concentrated the materials other than those found in olivine, namely, among others, the lime, alumina, alkalis, water, carbon dioxide, and uranium. The last stages of the solidification of the Earth, when the remaining liquid was only a few tens of kilometers deep, would, as is now known from the order of crystallization of rocks (cf. N.L. Bowen, Evolution of igneous rocks, Princeton Univ. Press, 1928), produce a layer of basalt and then a layer of granite, at the same time releasing the greater part of the water and carbon dioxide to form the primeval ocean and atmosphere. Moreover, at this last stage, crystallization at both the top and the bottom of the liquid layer would occur together on account of the lower temperature, increased viscosity, and the diminished opportunity for remelting of the crusts forming on the surface. The outer layer would have a variety and complexity in sharp contrast with the simplicity of the interior, and would resemble that honeycombed structure which Lord Kelvin postulated for the Earth as a whole.

Pressure--The pressure throughout the Earth is known more accurately than any other factor. Its magnitude at any depth depends very little on any particular hypothesis as to the origin of the Earth. Near the surface the pressure increases about 500 atmospheres for each mile increase in depth, or about 300 atmospheres per kilometer. Thus at 70 km the pressure is 20,000 atmospheres, which marks the limit of accurate high-pressure experimentation in the laboratory. At greater depths the pressure increases at a decreasing rate attaining at the center of the Earth a value of about 3,000,000 atmospheres.

Temperature--In contrast with the pressure, comparatively little is known about the temperature within the Earth. The temperature at any depth just after solidification was the freezing-point of the rock at that depth and, since only in the outer few hundred kilometers has there been any appreciable cooling, throughout the greater part of the Earth it is still at that temperature. If sufficient were known about the change of melting-point with pressure (or about the effect of extreme pressures on the volume-change and latent heat of melting, from which the change of melting-point could be calculated) it would be possible to specify exactly the temperature at great depths within the Earth. At the present time, however, all that can be done is to calculate approximately the temperatures at moderate depths. The results of such a calculation are shown in Figure 1 which probably represents as satisfactory an estimate as we can now make. The present temperature at any depth has been attained through gradual cooling from the freezing-point, the cooling being retarded by the heat generated by the small but important amount of radioactive substances within the rock, as suggested by Holmes (H. Holmes, Geol. Mag., v. 6, 2, 60, 102, 1915; H. Jeffreys, op. cit.).

The principal factors involved in the determination of these temperatures are (1) the age of the Earth, (2) the melting-point under pressure, (3) the surface temperature-gradient, and (4) the amount of the radioactive materials in surface-rocks. There is a regrettable uncertainty in all these factors especially the last two, but probably the uncertainties will not seriously influence one important result, namely, the amount of cooling at the present time below the melting-temperature or in other words the amount by which the temperature at any depth would need to be raised in order to melt the rock. According to Figure 1, it may be seen that at 100 km the cooling has been 450° and at 200 km only 300°.

Composition--From seismologic data it is found that the velocity of longitudinal waves in the outermost layer of the Earth--leaving sedimentary rocks out of account--is about 5.5 km per second which from laboratory measurements on the elasticity of rocks is the velocity which such waves should have

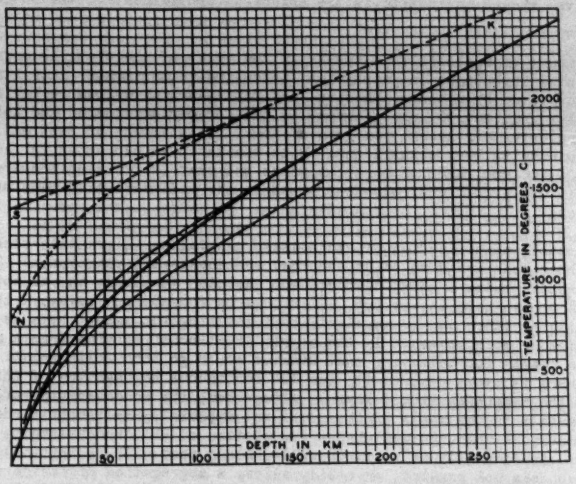

Fig. 1--The heavy line indicates the most probable temperatures at various depths down to 300 km (180 miles); the "initial" temperature is shown by the line NLK, but the line SLK leads to substantially the same result, because the present temperatures of the Earth are independent of the original temperatures in the upper 100 km

in granite. For this and other reasons it is believed that the upper layer of the Earth's crust is predominantly granite. This layer is probably irregular in thickness, which may vary between 10 and 30 km. Below this is a layer which has about the right characteristics for diabase or gabbro, while at a depth of 50 to 60 km a sudden increase in the velocity of longitudinal waves to nearly 8 km per second indicates a zone of a third kind of rock. Until recently the only silicate rock known to have a sufficiently high compressibility to admit of so high a velocity was peridotite, but measurements (L. H. Adams and R. E. Gibson, Proc. Nat. Acad. Sci., v. 15, 713-724, 1929) have shown that eclogite--a rock of the same composition as gabbro but crystallizing chiefly as garnet and jadeite--could also transmit longitudinal waves with a velocity slightly greater than 8 km per second, in striking confirmation of a prediction by A. Holmes. As far as direct evidence goes, it is not possible at present to determine whether peridotite or eclogite is the material existing at great depths, but various bits of indirect evidence give good grounds for believing that it is peridotite. In particular the average composition of stony meteorites, with their predominantly peridotitic character, give strong support to that view, if, as seems reasonable, we believe that there is only a very small probability that all the meteorites which have been found should happen to be made up almost entirely of a comparatively rare material.

The existence of a relatively thin layer of eclogite is, of course, not

excluded even though peridotite is the principal constituent of the whole Earth.

The amount of radioactive elements in the rocks of the crust is a factor of fundamental importance in geophysics. From the rather meager data available the heat generated by uranium, thorium, and potassium in various rocks is as follows: For granites 50, for gabbros 14, and for peridotites 4, the unit being 10^{-14} calories per second per gram of rock. This amount of heat while exceedingly minute is very significant. With the Earth as old as it is (certainly 1,000,000,000 years or more) it turns out that the present temperatures at any depth are almost entirely determined by the radioactive content of surface-rocks.

The notable concentration of radioactive substances in granitic rocks has sometimes been considered to be an abnormal and unexpected phenomenon. Uranium, for example, being a heavy element, was expected to settle to the bottom rather than concentrate in the top. It should be remembered, however, that in a liquid mixutre there can be no significant settling out except by a separation into two or more phases. In the crystallization of olivine from a magma there is no reason for believing that uranium would go into solid solution, and thus there is every opportunity for the continuous segregation of this element in the remaining liquid layer. Altogether, the concentration of radioactive materials in the rocks of the crust and especially in granitic rocks is no more remarkable than the well-known association of the rarer elements with pegmatites. Incidentally it is worth while noting that with an initially solid Earth, which does not require, or easily allow, a segregation of radioactive elements in the surface-layers, a radioactive content uniformly distributed throughout the whole Earth and of a magnitude no greater than found in basaltic rocks, it would follow that rocks even comparatively near the surface are far above their melting-point--a condition definitely contradicted by the known resistance to tidal definition.

Physical state--Probably the silicate part of the Earth is almost entirely crystalline. Localized regions of liquid material may exist but these must be of limited size. Similarly, thin layers of broad extent, in a glassy or even mobile condition, are not necessarily to be excluded.

Possible sources of liquid lava--At first thought it might seem difficult if not impossible to have in an Earth which has been cooling for one or two thousand million years any important amount of hot liquid or gas comparatively close to the surface--say within 50 or 100 kilometers. This, however, might happen in a number of ways, of which three will be discussed. First, there is the possibility of the existence of reservoirs of magma which had been entrapped by falling blocks during the last stages of the solidification of the Earth. If the liquid mass so entrapped were initially much hotter than the enclosing solid walls, and if the material is viscous enough to prevent convection, large liquid reservoirs would persist for a considerable fraction of the whole of geologic time. To calculate the cooling-rate of an irregular body is rather difficult but the order of magnitude for various sizes may be determined by considering spheres of various diameters. For a sphere of hot glass, which is initially uniform in temperature and the surface of which is maintained at a constant temperature, to cool down at the center by an amount which is one-half of the initial temperature-difference requires a time in years equal to 10,000 times the square of the radius in kilometers. Thus if the radius is 10 km, the time is 1,000,000 years and if the radius is 100 km, the time is 100,000,000 years. Of course, the condition of the constancy of surface-temperature is not fulfilled, but these values allow us to see approximately the conditions under

which magma-reservoirs might persist from the originally molten globe. It thus appears that without some source of heat only rather large reservoirs could persist and then only in early geologic times. In these early times vast outpourings of lava may have and probably did come from great pockets of residual liquid, but at the present time there is very little probability that the lava thrown out by a volcano comes from a reservoir of material which is and always has been liquid, unless there has been some other contributing factor.

Another way in which liquid may be supplied is by relief of pressure. As stated above, the temperature at depths of 50 km or more are not much below the initial temperature, which is the melting-point (at the pressure corresponding to 50-km depth) of peridotite. If at any depth the average temperature is above the ordinary melting-point of the rock which exists at that depth and place, and if by any means the pressure were released, part or all of the rock would melt, thus forming a supply of liquid. This melting by release of pressure has been discussed by Bowen (N. L. Bowen, op. cit., pp. 314, 317), who concludes that the necessary release of pressure is possible and that this factor may be of some importance in the production of liquid magmas.

The third source of liquid material is by local heating due to irregularities in the distribution of radioactive material. Remembering that according to our present views the temperatures at various depths within the Earth are almost entirely determined by the average amount of radioactive material in the crustal rocks, we see that a variation in the radioactive content at any place will make the temperature either greater or less than the average temperature for that depth. Even though the total amount of radioactive elements in rocks is very minute, a small increase in the relative amount at a given locality may raise the temperature above the melting-point and thus maintain in that region a supply of liquid almost indefinitely. Joly (J. Joly, The surface history of the Earth, Oxford Univ. Press, 1925) has developed a theory involving the heating and melting of basaltic material by radioactive disintegration followed by the cooling and subsequent remelting of the basalt. What we are considering here is a much simpler phenomenon. It does not involve widespread melting nor does it involve a periodic reheating. Unless there is a very uniform distribution of the radioactive elements throughout the rocks of the Earth's crust it seems inevitable that there should be regions in which the temperature is much higher than normal and that we have here an important mechanism for volcanic activity.

One objection to this view should be noted. When we examine the existing data on the radioactive content of rocks we do not find any excess for the rocks of volcanic regions and, moreover, regions which do show an excess may show no especial volcanic activity. If this result should be confirmed after extensive and accurate determinations of fair samples of the crust for some depth, we should have to conclude that the suggested mechanism does not exist. It should be borne in mind, however, that the amounts of radioactive elements in rock are very minute indeed and not as yet amenable to very precise measurement, and that for providing a supply of liquid it is required merely that in or near a volcanic focus the radioactive content be only half as much again as the average for the Earth's crust.

Summary

On account of the connection between the properties of deep-seated materials and the causes of volcanic activity, the present views on the temperature,

pressure, composition, and physical state of these deep-seated rocks have been summarized. Several possible sources of liquid lava within the crust have been discussed, and particular attention has been called to the production of molten magma by local heating in regions where the radioactive content is slightly above the average for the Earth's crust.

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